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TWO TELESCOPES AND THE NEW UNIVERSE

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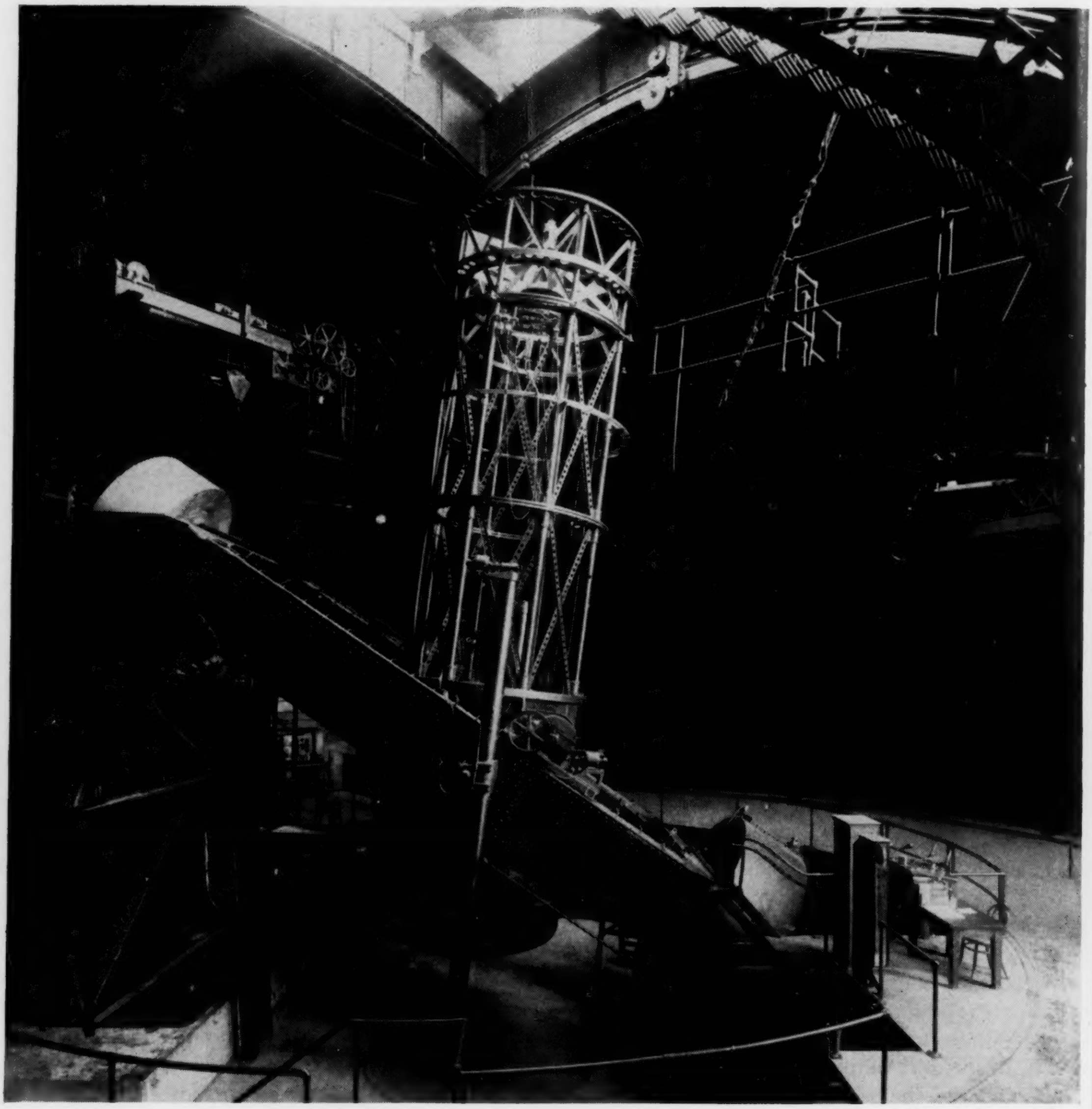
THE year 1948 marks the beginning of the reign of the new king of instruments on the throne of Palomar Mountain, California. Meanwhile, the royal predecessor, trapped on Mount Wilson near Los Angeles but still carrying on bravely over a flood of harrying light spilled out from the city, has already written into the history of human thought one of its major chapters. In its off-the-world affairs and speculations, the twentieth century is being dominated by two telescopes; and in my opinion the newest of these will be fortunate indeed to match the other by the yardstick of over-all accomplishment.

Nevertheless, the astronomical age now starting is one of justified optimism. Even the more thoughtful members of the human race, deadened as they are to the scientifically marvelous by having seen, in pictures at least, the very pillar of flame that dramatized the birth of the atomic age, await with calmness the first authentic report about an unexplored region some seven times larger than the present known universe. Surely this should be news of more than passing moment, even on such a crowded and self-centered planet as ours!

The time seems propitious, therefore, to set the stage for the interested onlookers. If we are to be prepared properly for the astronomical revelations likely to be forthcoming, we must have at least a rough understanding of those that have gone before. As befits the importance of the occasion, several articles dealing with some aspects of the unfolding story have already appeared. None of these, naturally, has exhausted the subject; and most of them seem to have plunged into the what-to-expect part of the discussion before the lay reader has had time to adjust his mental sights to the cosmic picture that is already before him. It will be a major aim of this article to convey to the reader some conception not only of the universe as it now stands revealed, but also of the tremendous change in outlook that has taken place between two world wars. And perhaps those last two words will explain in part the strange fact that few among the well-read people of this earth have the faintest idea why 1924 is one of the most important dates in the history of science.

Our discussion will be divided into three parts: The first will deal with the new telescope itself; the second and largest division will be concerned with the developing picture of the universe, which, incidentally, has swelled to new dimensions in my own lifetime and has culminated in the great instrument now almost ready to make the next enlargement; the third and final part will discuss the typically cautious predictions, made by the qualified astronomers who will carry on the work, with reference to the nature of the new things the telescope may reveal. I reserve the privilege, however, of supplementing these reasonably reliable forecasts with some less trustworthy speculations of my own (duly labeled as such) made in the light of my personal version of the major facts in hand.



Mount Wilson Observatory

THE 100-INCH TELESCOPE AT MOUNT WILSON

UNTIL 1948, THIS WAS THE LARGEST REFLECTING TELESCOPE IN THE WORLD.

THE spectacular success of the 100-inch telescope (of which we shall have more to say) resulted not only in a revolutionary new outlook on the universe, but also in an idea in the mind of one man that may prove to be of equal or greater importance. That man was the astronomer George Ellery Hale, and the idea was that of a telescope that would dwarf even the giant reflector on Mount Wilson. In 1928 the idea bore its first tangible fruit in the form of a six-million-dollar grant to the California Institute of Technology by the General Education Board of the Rockefeller Foundation. The grant was for the construction of a reflecting telescope with an aperture of 200 inches, to be built somewhere in the West Coast area.

The very conception of this plan, as a matter of fact, was more daring than it may appear in offhand retrospect. For the intricate engineering problems involved in the details of mounting and operating an instrument which combines the massiveness of a locomotive with the delicacy of a wrist watch are such as to make the addition of a few inches in the diameter of a projected telescopic mirror a matter for concern, long study, and techni-

cal ingenuity. But here the step-up was not of inches, but of yards! Here was the first call, with details as yet to be supplied, for a mirror with four times the light-gathering power of the greatest optical eye then existing—one able to reach galaxies roughly twice as far away as the most distant ones yet recorded, and therefore to bring into the realm of the explorable an approximate sphere of space about seven times as large as that now known to man.

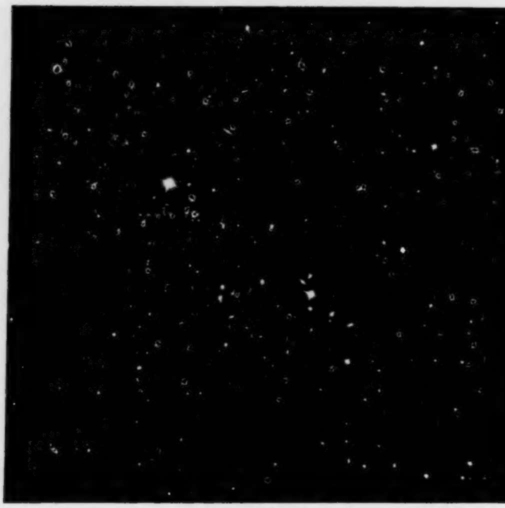
It is clear, then, that the task of designing and building a telescope of such unprecedented proportions was one not to be taken lightly. In fact, the very length of time involved in making the dream come true bears testimony to the unexpected problems that were met and overcome in the twenty-year-long epic of trial and error and vexing delay.

Of course, a world war and the consequent suspension of work on the project were added reasons for the excessive time consumption; and it probably is not true that at any time the final fulfilment of long-deferred hopes seemed even to border on the impossible. Given the necessary financial resources, it was what might be called a routine task of extraordinary difficulty which was eventually completed, just as those who were involved expected it to be. Nor was the delay an unqualified misfortune. Important advances of various kinds, particularly in photography, have called for modifications of the evolving design which take full advantage of new information and new techniques, so that the completed instrument is practically up to date in every respect.

With regard to personnel, Dr. John A. Ander-

NEW MONARCH OF THE SKY

SHOWING PRINCIPAL FEATURES OF THE NEW 200-INCH TELESCOPE, MINUS THE GREAT MIRROR, WHICH HAD NOT YET BEEN INSTALLED. AT THE UPPER LEFT IS THE CYLINDRICAL TUBE CONTAINING THE PRIME FOCUS, IN WHICH, FOR THE FIRST TIME, THE OBSERVER RIDES WITH THE TELESCOPE. *Left center*: MOVING PLATFORM FROM WHICH HE CLIMBS TO HIS POST.



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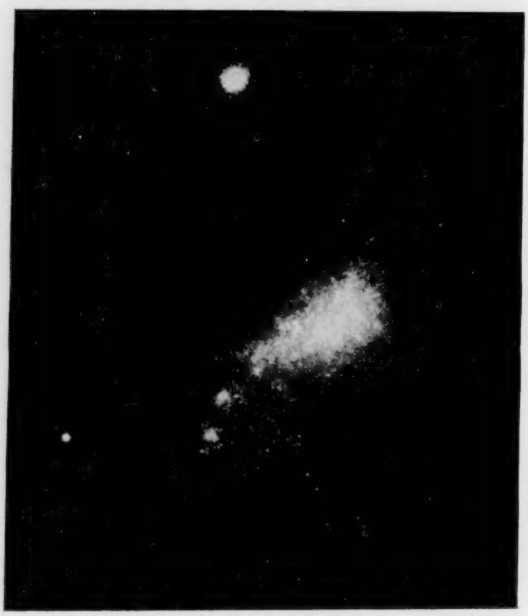
CORONA BOREALIS CLUSTER

A FEW OF THE HUNDRED MILLION GALAXIES THAT THINLY PRICK THE BLACKNESS IN THE PART OF SPACE THAT IS NOW REACHED BY THE 100-INCH TELESCOPE AT MOUNT WILSON.

son, of the Mount Wilson staff, has been in charge of the project from the start, and there has been excellent cooperation among the scientists concerned. In fact, it has been agreed by the directors of Carnegie Institution, which sponsored the bold adventure in applied science at Mount Wilson, and



Boyden Station, Harvard College Observatory

SMALL MAGELLANIC CLOUD

NAMED AFTER THE EXPLORER, THIS SEEMINGLY DETACHED PORTION OF THE MILKY WAY IS EASILY VISIBLE TO THE NAKED EYE FOR OBSERVERS IN THE SOUTHERN HEMISPHERE. IT IS, HOWEVER, ABOUT 80,000 LIGHT-YEARS AWAY.

of "Cal Tech," which received the Rockefeller grant, that the operation of the two great observatories should be a joint venture in research under a common director. That highly responsible position has now been filled by Dr. Ira Sprague Bowen of Cal Tech, a physicist and astronomer whose special field of "extreme ultraviolet spectroscopy and nebular spectra" carries a strong hint of the type of investigation that will most certainly be stressed.

There is not space here for the whole lengthy story of trial and frustration; of how, six years after the project started, at a time when it was to have been well along toward completion, the huge glass disc was finally poured at Corning, New York, only to break in its mold. Two years later the successful Pyrex slab, weighing 14.5 tons in spite of its ribbed and hollow construction, began its carefully routed journey to the laboratory of Cal Tech. There it was ground, tested, polished, retested, and repolished, in what seemed to be an interminable quest for perfection. From 1942 to 1945 the war called a complete halt, and then the polishing began anew. Finally, in the late months of 1947, the mirror was pronounced satisfactory, which meant that it missed parabolic perfection, in the roughest spots, by something less than a millionth of an inch.

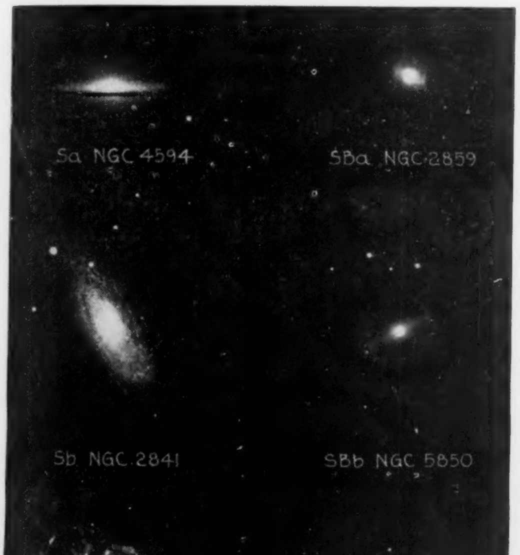
In the meantime, the long-sought site for the telescope and housing observatory had been agreed upon. Long before the last soft strokes were applied to the oversized jewel in Pasadena, preparations for its setting were complete on Palomar Mountain, which is a huge, flat-topped geologic block some 127 miles to the southeast. A two-million-dollar highway, 6.5 miles long, had been built along the side to the summit under the supervision of experts from San Diego, about 45 airline miles away. Now the beautiful dome of the observatory proper was complete, and so was the colossal telescope, except that a concrete slab still did balancing duty for the piece of glass on whose performance rested the whole fate and fame of the Palomar project. It is true that other astronomical work with other instruments was carried on in the far-from-idle community, which, incidentally, required a dormitory and seven cottages to house its personnel; but to the interested public, at least, nothing that mattered much could happen in advance of the Great Moment. That moment, of course, as envisioned by the more naïve, would occur when, with the great mirror in its appointed place, the slit of the dome would open majestically and some wide-eyed scientist, Adam's apple aquiver, would press his eye to the magic circle and shout excitedly at the marvels there revealed.

It is perhaps a pity that such a dramatic climax, after the long years of preparation, could not conveniently be arranged. Long before the dedication ceremonies the epoch-making First Glance was probably diluted and deglamorized in the course of several months of routine adjustments. Doubtless many have read that the 200-inch telescope is actually just a huge camera through which the working astronomer will seldom glance directly, and that its precious time will be devoted primarily to the study of distant galaxies. This is true, and yet not the whole truth. There is no optical law that will bar the use of the simple observing eyepiece, and there is probably no ethical code for astronomers and opticians that will prevent someone from taking an unofficial peek at even such an unworthy object as the moon. Perhaps, therefore, we can save a smudged corner of the romantic picture after all. Probably the First Peek, spiced with a bit of well-concealed excitement, has already gone into the side lights of history; and it even seems likely that some strategically placed feminine savant has learned by now what the New Look really means.

Boyden Station, Harvard College Observatory

LARGE MAGELLANIC CLOUD

THOUGH SMALLER THAN OUR GALAXY AND NEAR ENOUGH TO BE CALLED "SATELLITES," BOTH MAGELLANIC CLOUDS ARE PROBABLY INDEPENDENT GALAXIES. THEY ARE OF THE SO-CALLED IRREGULAR TYPE, THOUGH A HINT OF THE RELATIVELY RARE BARRED SPIRAL FORM MAY BE DETECTED HERE.



Mount Wilson Observatory

TYPES OF NORMAL AND BARRED SPIRALS

THESE ARE THE LARGEST KNOWN STRUCTURAL UNITS, EACH ONE CONSISTING OF MILLIONS OF SUNS. ABOUT ONE HUNDRED MILLION OF THEM ARE SCATTERED THINLY IN THE PART OF SPACE REACHED BY THE 100-INCH TELESCOPE.

And now, before we turn to the second phase of our story, let us take a peek of our own at the site of the unfolding drama, and at the mighty instrument that will play the leading role.

Why Palomar? In the first place, the mountain is not too far away from Pasadena and the interested scientists at Cal Tech and Mount Wilson. Of course, the site must be relatively high and in a predominantly dry, clear, calm atmosphere. Actually, the base of the telescope is 5,600 feet above sea level; and the air, which tends to cloud up and swirl about a sharp peak, is quiet on the broad, tree-covered top of Palomar. Then, too, the single mountain block is off to one side of the geologic fault where earthquakes are most common. Finally, it was desirable that the unreachable cone in space that surrounds the south celestial pole for observers in northern latitudes be reduced in size as much as possible; for that reason it was fortunate that this ideal location was rather far to the south.

As for the telescope itself, the bald figures involved are probably feeble substitutes for the actual sight of it. If, however, one tries to visualize a disc of glass which, with the sheathing around it, towers

vertically across two stories of a six-story building, and then pictures this as the moving end of a swinging, steel-girdered tube which, when upright, reaches to the top of this building, he may get a little of the feel of the proportions involved. Tonnage means little, but it may be worth recording that the telescope weighs 1,000,000 pounds altogether, of which some 280,000 pounds, or the weight equivalent of a healthy locomotive, are balanced so delicately that they move under gentle pressure. As in all large telescopes, the mounting is of the so-called equatorial type, which permits smooth, automatic, clock-driven following of a given celestial object. A less usual feature, however, is the horseshoe-shaped construction of the north end of the polar axis. This axis runs parallel to that of the earth, and in the case of the 100-inch it bars access to a limited region around the north celestial pole. The 200-inch, swinging northward inside the huge horseshoe, can point at the pole star and its neighbors at will. Thus, the new giant will have the advantage of a greater northern as well as southern reach in available area for study as compared with its nearest rival, in addition to its well-known advantage in the matter of space penetration. And when we consider the mighty deeds of the deposed champion eye of the world, made without the technical refinements of the past three decades, the prospects for new conquests by the favored successor look bright indeed.

The accelerated tempo of events on earth, in the span of years between two world wars, has been more than matched in the realm of ideas. In physics and chemistry, the universe of the minute has been unlocked for all to marvel at, and the whole world knows of the atomic bomb. In astronomy, the steps have been equally gigantic; but the advertising department has fallen down on the job, and the man in the street—even the supposedly well-informed one—has had little inkling of the things that were going on inside the nonpractical heads of the stargazers. He would be surprised, and probably skeptical, if he were told that in his own lifetime, and in part at least through the agency of the 100-inch telescope, an Age of Revelation has torn away a major veil from the face of the universe itself.

He knows about the intellectual revolution symbolized by Copernicus the thinker and Columbus the actor some four and a half centuries ago, when to an ever-swelling group of knowing ones the stationary earth rounded into a spinning ball, and the near-by stars, now stilled in their "crystalline sphere," moved backward and outward into space. Those were the stirring days when the early giants of modern thought, from Galileo to Newton, began to roil the waters of complacent stupidity with such mighty splashes that even the slumber of the masses was disturbed. This is history; and this is moderately well known. But how many realize that a mere quarter of a century ago, in the age of flappers, gangsters, and diamond-studded pocket flasks—during, in fact, the course of a single year or two centering about 1924—quiet announcements were being made that deserved to ring out in the world of science like the notes of a cosmic bell. One of the two or three great hours of human awakening came and passed; and then Coolidge did not choose to run, the stock market crashed, and the dying echoes of a scientific rumor, little heeded at any time, faded away at the sound of footsteps in the bread lines of the world.

Perhaps this sounds like journalistic exaggeration rather than an attempt to evaluate the event described in careful and precise language. It is true that there has been no restraint, and that I have used all the emphasis at my command to call attention to one of the greater moments in the history of human thought. Is this an unwarranted pose—an overrating of one discovery among a dozen others of like importance? Let us see.

At this point the injection of a personal note may help to bring full realization of the change of outlook that has taken place in one lifetime. To make it clear, however, that my judgments are impersonal and objective, let me say at once that I have been merely an interested nonprofessional observer in this field, and am speaking as a reporter rather than as one who has had even the tiniest of roles in the drama itself.

In 1916, when as an undergraduate at the University of Minnesota I was absorbing the current picture of the universe about me, the scene was vast even then, though perhaps a bit dismal. Well do I remember the stirring feel of distance and emptiness and a far-flung universe of suns—of remote, solitary, and majestic orbs which were like the sands of Sahara in number, but which faded in the farther zones to sparks in silhouette against the blackness of engulfing space. For here was the central fact of the universe as it then was known: the ranks of the stars were thinning as men plumbed the outer reaches of space. Black holes in the Milky Way served as windows through which we could look outward and into the void. It was not altogether a pretty picture.

These were the facts of observation, though it is not to be inferred that no one suspected the grander view that was to come. Already a number of fine photographs of the so-called spiral nebulae

had been made, particularly by Keeler; and there were those who argued that the spirals were themselves great systems of suns like the stellar horde that we see from the inside as the Milky Way. The matchless modern observer, E. E. Barnard, who photographed the Milky Way and described it beautifully, believed correctly that the apparent holes were not windows at all, but dark patches of intervening cosmic dust which blotted out most of the light from the stars beyond. But these were still just theories, and for all anyone knew the spiral nebulae were like the other huge blobs of rocks and dust in which some of the stars are suspended. In fact, the very name "nebula," or "cosmic cloud," testifies to this point of view. They could be of the order of size of the Great Nebula in Orion, which is only ten light-years, or some sixty millions of millions of miles, in diameter. They could be mixed with, and lighted by, some of the stars in the one observed system, though certain peculiarities in their distribution made this conclusion seem doubtful to some. Nevertheless, the one great generalization of the day, which came out of patient observation and keen deduction, was that of the ultimate exhaustion of the starry horde in the distance. This, in the larger aspects, was the state of astronomical science in the first two decades of the twentieth century.

What happened then? We must skip most of the details; but one significant event was the building of a reflecting telescope with an aperture of 100 inches, at Mount Wilson, California. Another was the more or less casual observation that a certain type of variable star called a "Cepheid" (so named from the constellation in which one appears) gives away its distance as compared with fellow-Cepheids in a very simple manner. This particular type of star changes in brightness at regular intervals, and the longer the interval between "winks," the greater is the actual or intrinsic brightness of the star. This observation was made by Miss Henrietta Leavitt, of Harvard Observatory, with regard to the Cepheids in the smaller Magellanic Cloud, a seemingly detached portion of the Milky Way, visible in far southern skies, whose stars are all roughly the same distance away. Later, this very important "period-luminosity relation" was confirmed and extended in a program which involved many observatories and astronomers, and for which Dr. Harlow Shapley deserves much of the credit. The suns involved were giant ones that could be seen as individuals at tremendous distances. The links in the chain of evidence were being forged. And then, late in 1924, the pieces of the jigsaw

Mount Wilson Observatory

TWO VIEWS OF JUPITER

Top: MARCH 15, 1921, WITH GANYMEDE AND SHADOW; *bottom:* MAY 29, 1922. (100-INCH HOOKER REFLECTOR).

puzzle came together and the new cosmic picture stood revealed, consistent in outline, grand in contour, and surprisingly indisputable, once it was seen.

The deciding factor was the new high in resolving power reached by the then recently completed 100-inch telescope. This technical term, incidentally, means the measure of a telescope's ability to show as separate individuals two stars that are optically close together. The larger the diameter of a telescope, the greater its resolving power. On a Mount Wilson photograph of one of the larger spirals in area, and therefore presumably a nearer one, some individual stars were resolved in the fringes away from the center. Some of these in turn were Cepheid variables whose distances could be determined. The cat was out of the bag. They were nearly a million light-years away! The Great Nebula in Andromeda differed from the gaseous cloud in Orion as a forest fire differs from the blaze of a match; all by itself it was a vast stellar system

like the only one we had known. The island-universe theory was now demonstrated truth, and some hundred million galaxies comparable with our Milky Way stretched outward to the borders of telescopic sight. The universe according to man's understanding had exploded into a new order of size.

Have I then exaggerated the importance of the intellectual revolution of 1924? It does not seem so to me. Remembering that the 200-inch telescope will give us only eight times as much observable space for study as we had before, we should be warned not to take for granted new revelations of the order of those that already have come in our lifetime; and at the same time we should be made somehow to realize that it was the 100-inch telescope that played a crucial part in bringing to pass the modern age of miracles in the field of science. Certainly only one other development of our time is comparable at all with the discovery of 1924. In practical consequences, the release of atomic power is of course much more important; but from the point of view of pure science and of sudden access of knowledge, the two great events are probably of the same order of magnitude.

And now let us consider the astronomical picture as of today, as the 200-inch giant moves into the scene. As in the previous description of the astronomer's view in 1916, we shall glance only at the high lights of the scene, now vast beyond all imagining. There are exactly two of these outstanding, all-encompassing features; and there would probably be little argument among the astronomers of the world as to what they are. It is only when we turn to the innumerable details that differences of opinion begin to appear.

Fact No. 1 is that in the approximate sphere of observed space there is uniformity in the large

THE GREAT NEBULA OF ORION

FEW PICTURES TAKEN ON OUR PLANET CAN RIVAL THIS ONE FOR SHEER MAJESTY. HERE THE DUST AND DEBRIS OF A EXCITING REGION ARE BATHED IN THE LIGHT OF ENVELOPED SUNS. THOUGH VAST BEYOND ALL IMAGINING (PERHAPS LIGHT-YEARS IN DIAMETER), IT IS AN INSIGNIFICANT DETAIL OF OUR GALAXY.

Mount Wilson Observatory

PART OF THE GREAT NEBULA OF ANDROMEDA

ONE END OF THE FAMOUS NEBULA, WHICH IS A NEAR RIVAL OF OUR OWN GALAXY IN SIZE (THOUGH SMALLER), AS WELL AS A NEAR NEIGHBOR IN DISTANCE. THIS VIEW SHOWS SOME OF THE INDIVIDUAL SUNS WHOSE VARIATIONS IN BRIGHTNESS GAVE AWAY ONE OF THE GREAT SECRETS OF THE AGES. (TWO HOURS' EXPOSURE, 100-INCH TELESCOPE.)

Galaxies are no more and no less numerous near the rim than they are at the approximate center, say, within the small globe containing no points more than fifty million light-years from the sun. But our use of that casual figure instead of, say, five million light-years, is not at all accidental, since the latter choice would have named a sphere containing a local condensation of galaxies. It is as if a giant hand had reached in among the pinwheels of space and pulled them, here and there, into more or less isolated flocks, leaving the larger-scale distribution unchanged. And the uniformity applies not only to the grosser features of mass and structure, but also to chemical composition, as we learn through the magic of the spectroscope. The universe of the 100-inch telescope is one grand unit; and this basic generalization of twentieth-century astronomy should be kept in mind when the new picture begins to unfold.

Fact No. 2 is the mysterious phenomenon variously described as "the red shift" or "the expanding universe." It is concerned with the so-called Doppler effect, which changes the number of vibrations per second that reach us from an approaching or receding body. In the case of sound, this makes the pitch of the whistle blast from an approaching locomotive higher than it is when the train moves away. In the case of light, it causes the lines in the spectra of stars to be shifted toward the red or the violet end according as the source of light is going away from or coming toward the recording spectroscope. And for the visible universe as a whole, the message of this instrument is a most astounding one. Apparently, the farther away a given galaxy is, the faster it is moving outward into space! Or perhaps this is not the case at all. At any rate, the indisputable observed fact is this: the farther away the galaxy, the more the lines in

its spectrum are moved toward the red. And if this shift is explained as it is in the case of relatively near stars, for which the interpretation in terms of radial motion is almost certainly correct, it would mean that a fantastic and almost unbelievable situation is revealed by the evidence. It would mean that some two billion years ago—which is to say, practically yesterday as the life spans of suns are reckoned in the best astronomical circles—an explosion took place in the universe that must have surpassed even a Cecil B. de Mille spectacle. The fastest moving clouds, or galaxies, are naturally by now the farthest away. The whole thing is implausible to the *n*th degree, utterly absurd, utterly ridiculous; but there it is.

Here, then, is fact No. 2 about things as a whole; and the fact is the prime mystery of the age. The available explanation in terms of known principles of science leads to the uneasy conclusion that the universe is fading away into endless reaches of nothing at all. I for one am not ready to go along with this thesis. I feel much more friendly to the idea that somewhere in the woodpile of theory dignified by the name of the expanding universe there is a very large and very obnoxious Ethiopian, but of course this is a purely personal reaction. Fortunately, the time is coming when no one will have to rely upon such dubious guides as our own hunches about the fitness of things.

Before leaving the picture of the observed universe as it now presents itself, we should perhaps mention one important but secondary item of information. As of 1916, the grand orbit of the sun, if any, about some more massive body, if such there were, was totally unknown; and I remember that the quest for such information was considered practically a hopeless undertaking. Today, the facts on this point are fairly well established: the orbit (not to be confused with the twelve-miles-per-second local movement of the sun with reference to neighboring stars) is a grand ellipse some seventy thousand light-years across; the massive attracting body is the sum total of suns and matter in the heart of our Milky Way galaxy; our speed in this cosmic Derby is of the order of two hundred miles per second; and the period of revolution is roughly —very roughly—some two hundred million years. Thus has science progressed in some thirty short years! Of course, fact No. 1 is still grand-scale uniformity, and fact No. 2 is apparent outward motion; but this lesser fact has also some large-scale aspects. The spectroscope shows that other galaxies, too, are making giant revolutions. The stars, like our sun, and the planets, like our earth, share this type of motion, as perhaps do the electrons about their protons and neutrons, and some still smaller unknowns about the nuclei of unguessed subatoms. Truly, on any scale, we live in a whirling universe.

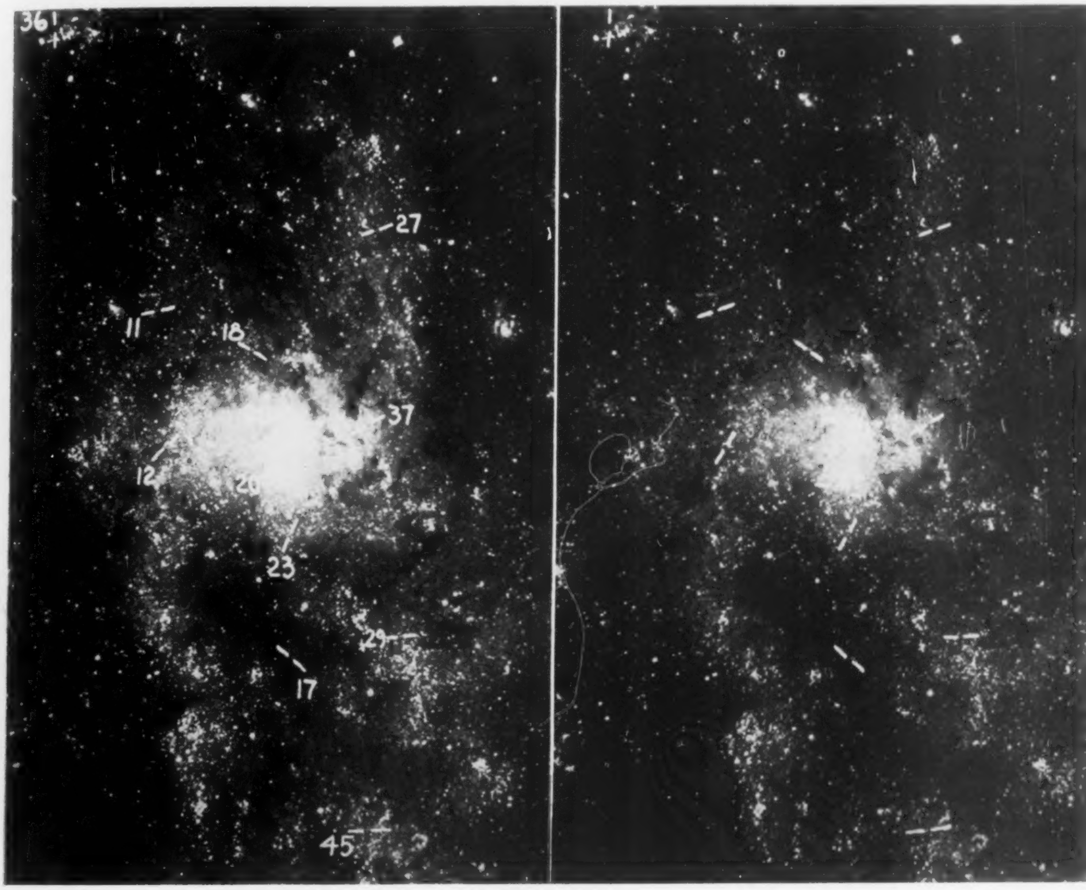
With this background in mind, it should not be hard to see what are the primary questions that the new telescope will answer, or at least cast new light upon. They have to do chiefly with the nature of the enlarged universe as a whole, since the task for which the big mirror is best fitted is not that of magnifying objects such as near-by planets. Actually, its main job will be that of gathering light from suns and galaxies too far away, or at least too faint, to be studied well with other instruments.

With regard to the first observed fact that the universe is one spread-out unit, the basic question is simply: "Is this a true sample?" If the answer is "No," the telescope may settle the matter; if "Yes," the best it can say is "Maybe," or "So far, it appears to be." In any case, the answer is likely to come slowly, based as it will have to be, upon patient, statistical investigation. The answer it gives, moreover, is tied up with the second main fact concerning present astronomical knowledge, with its inevitable accompanying question: "Is the universe really expanding?"

Here we are on somewhat more optimistic grounds, at least with regard to the likelihood of a positive answer. Indeed, Professor E. P. Hubble, whom many will regard as the leading authority in this field, has reached such heights of confidence that he has, so to speak, put himself out on a scientific limb. He predicts rather flatly that the new telescope will give a definite answer. By that he means "ultimately," of course—not necessarily in 1948. For, whether the cause of the red shift be actually a flying away of the galaxies—and "flying" is an understatement—or a change in the quality of light due to "travel-fatigue," the added reach of the new telescope should supply enough data to provide the answer. In this connection, Dr. F. R. Moulton suggested long ago that according to Planck's equation, $E = hc/\lambda$, in which h is Planck's inviolate constant, c the fixed velocity of light, λ the wave length, and E the radiant energy, if there is even the slightest toll charge on E for an eon-long trip through the rare and minute obstacles of intergalactic space, it must be paid by means of an increase in the wave length λ. If so, all this fuss about an exploding universe may, after all, be quite unnecessary.

As to what will be learned in the investigation, there is still another possibility, commonly described somewhat as follows: Space may be

"curved," and the new telescope may reach far enough toward the limits of the bounded universe to make the curvature apparent. In that case, the curvature would be "positive," as suggested by the surface of a sphere; in an alternate situation, it might be "negative," like the face of a saddle. But whether such attempts to "explain the unexplainable" by the use of vaguely suggestive and possibly misleading analogies actually do skirt the fringes of ter in the universe is finite, a ray of light will not go on forever in a straight line, but will describe a giant curve concave to the bulk of the galaxies. If that be true, then we can never hope to see anything outside the vast but limited pile of stuff that we are pleased to call "the universe," though for all we know there are other piles of stuff, with other self-enclosed light systems, in other remote places. In any case, it would seem more honest, or



TWO EXPOSURES OF THE SPIRAL TRIANGULUM

A TYPICAL SPIRAL. THE BARS INDICATE VARIABLE STARS. NOTE, FOR EXAMPLE, THE STAR MARKED "17," WHICH IS MISSING IN THE LEFT PHOTOGRAPH, BUT FLASHES OUT IN THE RIGHT ONE. MOUNT WILSON OBSERVATORY PHOTOGRAPH.

reality and point the way to some deep and obscure truth, we shall leave to those who wish to grapple with such problems.

With full knowledge, then, that we are omitting some aspects, both mystic and mathematical, of the idea of "curved space," we shall consider one relatively simple related thought. This thought is that a ray of light bends somewhat, just as does the path of a thrown stone, when it passes a massive object. Einstein predicted this, and the evidence seems to bear him out. If, then, the amount of matter in the universe is finite... at least less misleading, to describe this possibly limited realm that man can see as "man's visible domain" rather than "the bounded universe."

And here we get into the semantic anarchy that delights the hearts of some popular expositors of science. A "straight line" is after all fundamentally a mental concept, difficult to define but easy to understand in its ordinary connotation. Since a definite concept of its meaning is shared by intelligent laymen as well as by mathematicians, it seems that a straight line should be entitled to re-

main straight in the interests of mutual understanding and priority of definition. This means that the two far ends should never meet, whatever the universe may be like. But what happens? Instead of making the simple statement that a ray of light does not move in a straight line—an assertion which, true or false, would be easily understood by those who remember their Euclid—some thinkers in this field prefer to *define* a straight line as the path of a light ray, come what may. Thereby they are led, logically enough, to such subtleties as "curved" and even "non-Euclidean" space. Their straight lines run straight in a bending "space"—and another good word which was formerly accepted as meaning something at least vaguely comprehensible goes into Websterian hysterics. But of course all this is chiefly a quarrel with words rather than ideas, and should not be taken too seriously. Speaking for myself alone, I have always felt that technicians should invent technical words for technical uses, and not confiscate and revamp the old standbys.

Having disposed of the less reliable personal reactions which were predicted in the introduction, we return to the 200-inch telescope and point out that one of its major accomplishments, totally uninfluenced by advance human surmises, may be to verify the truth of the statement that man's *access* to the universe is limited, or, on the other hand, to indicate that unreached space is much like the sample at hand, and that actually the man-sensed universe may stretch outward without end.

Coming down out of the ether of speculation about the facts of cosmogony and the shape of the universe, we can say with certainty that the primary service of the 200-inch telescope will be that of funneling light, in larger quantities than formerly, into suitable apparatus for detailed study. The receiver may be a photographic plate to record magnified features of a galaxy or the distribution of a star cluster, a bolometer or thermocouple to measure the temperature of a light source, or perhaps a photometer to calibrate brightness. Finally, of course, the receiver may be the almost miraculous spectroscope, from which one can learn, among other things, the chemical composition of the source of light, its motion, mass, magnetic condition, cosmic age, or what you will. In fact, in the light of past accomplishments, it seems a bit rash to deny the possibility that the spectroscope will tell us, ultimately, the innermost thoughts of some hypothetical dwellers on unseen planets circling a middling sun in one of the remoter galaxies. At least we shall surely get much more information about many things, both far and near. Among the likelier prospects are the dwarf suns in our own celestial neighborhood—those worn-out dispensers of faint light that are assuming a role of increasing importance in modern astrophysical studies.

Finally, in spite of the fact that the new tool of research is not designed primarily for investigations within the solar system, it does not follow that the telescope will never be used for this secondary purpose. In fact, some plans have already been made to use its great light-gathering power for the purpose of reducing the time of exposure for photographs of Mars. It is believed that in a long series of repeated flash shots there are likely to be some lucky ones, taken in rare moments of atmospheric calm, which will settle once for all some long-debated questions, such as those concerning the straightness and artificial character of the canals on Mars. It is not improbable that some of the more spectacular of the achievements of the new telescope, at least from the standpoint of popular interest, will deal with matters in our own tight little solar family.

In summary, then, the 200-inch giant of Palomar Mountain will give us more data on the question of whether or not the universe around us, with its millions of galaxies lighting the void as far as we can see, is a fair sample of a larger reality. It will probably enable us to decide definitely whether or not this welter of cosmic pinwheels is actually fanning out, as it seems to be, in a vast pyrotechnic explosion. It *may* tell us whether the borders of our enlarged domain are drawing near to the limits of human reach; and it surely *will* throw much more light on the chemistry of all things, far and near. Our knowledge of the solar system will probably be enhanced, and we may get some important information on the problem, or at least the distribution, of life itself.

It is perhaps too much to expect that this new masterpiece of engineering will bring about an intellectual readjustment matching in spectacular quality that which was sparked by the Mount Wilson giant; but even that may happen. After all, it seems wise not to discount too much the potentialities of the atomic age.

NAMING THE GARDENIA

MARGARET DENNY

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NOT content to be merely armchair scientists, many "curious" men of the Enlightenment—lawyers, merchants, country gentlemen, ministers—felt such enthusiasm for the methods and objectives of science that they wished to take an active part in the scientific work being done at that time. Their cooperation was welcomed both by the Royal Society of London and by master scientists who had need of many helpers. Two hundred years ago the word *virtuosi* was used to describe such men. Those amateurs who were able successfully to apply the Linnaean principles of classification were encouraged by the great Swedish botanist to send to Upsala accounts of all flora and fauna believed to be "nondescript." Alexander Garden, a physician of Charleston, South Carolina, was numbered among the so-called "sons of Linnaeus." In 1760 Linnaeus named a certain genus of plants *Gardenia* in his honor, and in later years Garden sent his master so many specimens of various kinds that Linnaeus called him "the happy illustrator of Nature in his own region of Carolina."

In the naming of the gardenia, the behavior of the most renowned of all botanists was both informal and charming. The central character in the story is, of course, Dr. Garden; but another virtuoso, John Ellis, who was a London merchant and the friend of both Linnaeus and Garden, has an equally important role. The story also concerns four others: the daughter of Cadwallader Colden of New York, who shared her father's botanical interests; Mr. Richard Warner, whose garden contained an "elegant plant;" the superintendent at Chelsea Garden, who was the author of a botanical dictionary; and Mr. James Gordon, a famous London gardener. The Duchess of Portland is a mute actress in the drama of the gardenia; one element of the plot is the sum of five hundred pounds.

Such a story is worth telling for its own sake, but it also deserves attention because it explains why the eighteenth-century virtuosi found botany such an absorbing avocation; it reveals the satisfactions to be derived from association with the great Linnaeus and with fellow-amateurs.

Nothing in the story of the gardenia is more interesting than to observe the intimate place science had in the private lives of all the people concerned. Factors in human personality were constantly affecting their scientific work and attitude. To these virtuosi, even to Linnaeus himself, the pleasures of science and of friendship were identical.

Various motives prompted the virtuosi to aid in the progress of science. One important spur was the popular eighteenth-century belief that science strengthened religious faith and induced a growth in religious sentiment. Another was the social concept of science. Like Sir Francis Bacon, these men believed that the end of science was "the relief of man's estate." The actors in the story of the gardenia shared these beliefs, but apparently such assumptions were too familiar to warrant their comment upon them.

Certainly human pride had some part in causing the sons of Linnaeus to search for new plant genera. It was his practice to name such plants in honor of the promoters and benefactors of botany. No little fame was enjoyed by an eighteenth-century man whose "name-sake" was listed in the *Systema Naturae*. The virtuoso who was known to be the correspondent, as well as co-worker, of the master of Upsala was the object of considerable envy.

Linnaeus very wisely permitted the discoverer of a new genus to suggest the name he wished given his plant, and when possible that wish was honored. The amateurs could use this opportunity to compliment a fellow-botanist, cement a friendship, find a new way to pay old debts.

The association of the London disciples of Linnaeus differed somewhat from the conventional club life so popular during that period, but it furnished similar social pleasures because there was an opportunity at public and private meetings to discuss moot points in botany and a chance for an exchange of visits to the gardens of fellow-ama-

teurs. For the botanical virtuoso who lived in America, however, the only substitute for such "instructive conversation" was correspondence.

After the completion of his education in Scotland, Garden returned to Charleston in the early fifties and was dismayed to observe "there is scarce one here that knows a cabbage-stock from a common dock, but, when dressed in his plate, by his palate." When in 1754 Garden became a Linnaean "convert," he felt keenly the need of counsel and encouragement in his new studies. As quickly as possible he established correspondence with kindred spirits in the other colonies and across the seas.

Garden's correspondents must have felt at once the energy and enthusiasm of the man. He exhibited a true flair for applying Linnaean principles, and his attitude toward botanical study was one of high seriousness. He showed himself master of the cooperative method. Obviously, such a man was deserving of compliment by his fellows and of public recognition as a worthy disciple of Linnaeus. It is not surprising, therefore, that this story will be concerned not with one "gardenia" but with many.

Ellis' first gardenia. Garden began his correspondence with John Ellis in 1754. There followed the characteristic give-and-take of a friendship between virtuosi. Garden sent American seeds to Ellis, which were then planted in his garden or distributed among Londoners of his acquaintance. Garden cooperated in Ellis' experiments with improved methods of shipping seeds; he forwarded shipments of plants addressed to Ellis from Florida. Ellis in turn transmitted books and botanical gossip to America, and as a Fellow of the Royal Society he was able to publish in *Philosophical Transactions* descriptions of new plants Garden had discovered. Then, too, after Garden began writing to Linnaeus, Ellis forwarded the letters and specimens from America bound for Sweden.

In 1756 Garden sent Ellis descriptions of six of Carolina's plants. These both Ellis and Garden were anxious to have appear in print because publication usually prevented other men from laying claim to the plant or its name. Ellis, however, wanted to make doubly sure about Garden's plants, and he therefore sought the advice of Linnaeus. "I am very sensible," Ellis wrote in the letter that began his long correspondence with Linnaeus, "your judgment in these matters far exceeds either the english botanists or those of any other nation." Linnaeus gave one of the American plants "the bishop's touch," as it were, and together Ellis and Garden named the genus *Halesia* in honor of the man whose name Garden had used when first addressing himself to Ellis.

Among Garden's six genera was a plant commonly called "Buereria;" but because Ellis knew it carried other names as well, he apparently posed some questions to Linnaeus about this plant. The reply of Linnaeus is not extant, but in May 1757 Ellis wrote:

> You desire my advice in the affair of the Butneria and Buereria. What you say is very true. Call it which you will you will certainly give offense to one or another. Mr. Miller has called it Basteria. But if you will please to follow my advice, I would call it Gardenia, from our worthy friend Dr. Alexander Garden of S. Carolina, who will take it as a compliment from you, and may be a most useful correspondent to you, in sending you many new undescribed plants.

Ellis was not to learn until the appearance of the tenth edition of the *Systema* in 1759 that Linnaeus had not taken his advice.

Miss Colden's gardenia. Garden first met Miss Jane Colden in 1754 when he visited the country estate of Cadwallader Colden, of New York. He sent Ellis a full account of his trip to Coldengham. "Not only the doctor himself is a great botanist," wrote Garden, "but his lovely daughter is greatly master of the Linnaen [sic] method and cultivates it with great assiduity." Her admirers were numbered among the virtuosi overseas also. In 1758 Ellis sent Linnaeus one of Miss Colden's plants and wrote:

> This young lady merits your esteem, and does honour to your System. She has drawn and described 400 plants in your method only: she uses English terms. Her father has a plant called after him Coldenia, suppose you should call this Coldenella or any other name that might distinguish her among your Genera.

Peter Collinson's comment to Linnaeus made in the same year is well known. "As this accomplished young lady is the only one of the fair sex that I have heard of, who is scientifically skilful in the Linnaean system," he said, "you, no doubt, will . . . recommend her example to the ladies of every country."

If Miss Colden provoked such flattering sentiments from those who had never met her, it is easy to understand why she deeply impressed the young naturalist and bachelor from Carolina who was visiting at her home. Garden's letters—all addressed quite properly to Cadwallader Colden—came thick and fast in the months immediately following his departure. In January he reported that he had written from Philadelphia on his way south and had sent another letter a few days after his arrival home. In February he sent Miss Colden some Persian seeds "got from Mr. Mounsey chief

Physician to the Army & Physician to the Prince Royal of Russia." "I shall in my next," he promised, "mention to Miss Colden the method of preserving Butterflies Eccc."

Clouds appeared on the horizon in May, however. Garden wrote to Colden:

> By your second letter I find that I have very innocently offended both you & Miss Colden in some expressions that insensibly dropt from my pen as archetypes of what my heart dictated in was on sincerity. This gives me real concern & give me leave to assure you I shall endeavour as far as in my power to amend any thing in my conduct or manner of writing you are kind enough to point out as wrong. I trust that both you & your daughter will forgive me for once, I shall be more sparing in saying what I think is due to such merit for the future—The expression which you say gave her most offence, gives me now a great deal of uneasiness as I suspect it has deprived me of the pleasure of a letter from her by last opportunity.

The precise nature of Garden's unseemly behavior can only be surmised. Perhaps Garden misinterpreted the temper of Miss Colden when she expressed the desire to give his name to one of her new plants. It is fortunate that Colden granted Garden permission to tell this news to his correspondents, because Garden had immediately written to a friend in Edinburgh about the plant and its proposed name. Early in the following year Garden told Ellis: "By a letter from Dr. Whytt, I learned that they had published the characters of a plant which I had sent me by Miss Colden, which she had called Gardenia."

The episode closes at this point. Linnaeus never adopted one of Miss Colden's genera under this name, and Garden's marriage to a Miss Peronneau of Charleston took place on Christmas eve, 1755.

The gardenia of Alexander Garden. Linnaeus was forced in 1758 to reject a plant sent him from Carolina, but he wrote to Ellis at that time, "If Dr. Garden will send me a new genus, I shall be truly happy to name it after him, Gardenia." Naturally, Garden wished to take advantage of this flattering offer. Within a few months he submitted specimens and description of a "very pretty shrub" he had come upon three years before and he expressed the hope that the plant would find "its proper place in your Systema Naturae." A few weeks later he wrote to Ellis:

> I have sent him a new genus, which he desired I would, that he might name it after me, but that I have left to himself, as I am very sensible I am yet rather too much of a Tyro to have that honour conferred on me.

Linnaeus offered no comment on this plant to either Garden or Ellis, and as late as the end of 1762 neither virtuoso was aware that it had already been described and named.

Ellis' second gardenia. Early in 1758 Ellis became interested in a plant called the "Cape jasmine," which he found growing in the garden of Mr. James Gordon at Mile End. He heard that the plant had been brought from the Cape of Good Hope four years before, but its owner, Mr. Warner, had been unable to cultivate it. Although the other London gardeners had failed in their attempts, Gordon successfully raised the plant from cuttings. It was classified as a jasmine by Mr. Philip Miller, superintendent of Chelsea Garden and author of a botanical dictionary. Ellis, however, believed the plant was new, nor was he impressed by the authority of Miller.

One day Ellis, accompanied by Peter Collinson and Georg Ehret (famous for his drawings in Linnaeus' *Hortus Cliffortianus*), went to "Mr. Warner's, a very curious gentleman, at Woodford near this City, to see his rare plant like a Jasmine, with a large double white flower, very odoriferous." The men induced Warner to let a dried specimen be sent to Linnaeus. Then Ellis, in the presence of Linnaeus' friend Dr. Bierken, dissected the double flower and sent the description to Sweden. "If you find this plant to be no jasmine, but an undescribed genus," Ellis told Linnaeus, "you will oblige me in calling it Warneria after its worthy possessor."

Linnaeus was certain it was no jasmine, but he was loath to offer further judgment as long as he could examine only the double flower. "May I be allowed," he asked Ellis, "to insert the character of this genus, on your authority, in my Systema, as I am unable to make it out myself?" On further consideration, however, Linnaeus decided he would "rather not meddle with this plant at all, till it is better known."

Then came the Christmas holidays, and something happened to Linnaeus that will strike a familiar chord in the heart of many a college professor.

> I had always present in my collection a dried specimen, with a single flower, preserved in some part of my herbarium, which, as far as my memory would serve, I believed to be the same species. Many a time, in the course of the last half year, have I hunted for this specimen, but in vain, my thoughts having been so much taken up with the daily business of the University, my publick and private lectures, and numerous matters besides.
>
> During the Christmas holiday, however, having found leisure to turn over all my dried plants, I luckily met with the specimen in question. The flower is perfectly single, and by immersing it in hot water, I could clearly ascertain every part of its structure, so as to draw up, without any uncertainty, the following characters of your genus Warneria.

To disentangle one snarl, however, was but to

encounter another. Suddenly the plant was without a name because the "very curious gentleman" at Woodford had announced that he did not wish the plant to be called "Warneria." Ellis wrote to Linnaeus:

> I believe he is convinced that it differs from the Jasmine; but he has such an esteem for Mr. Miller, that he would not appear to differ from him in so capital a plant by adopting another name.

It is not strange that today we cannot understand why Warner made such a decision; the years that have elapsed since that time have brought about such a radical change in popular scientific thought that even the eighteenth-century meaning of the word *virtuoso* has become obsolete.

Warner was certain to gain in reputation among his fellows if Linnaeus named the genus "Warneria" and published the news in his *Systema*, nor was Warner loath to accept this signal honor that only Linnaeus could confer. But, as the personal friend of Philip Miller, Warner had concern for Miller's reputation among Londoners as a botanical authority. The linking of Warner's name with the new plant genus was certain not only to weaken Miller's prestige, but also to give public announcement of Warner's disagreement with his good friend. Warner was confronted with the hard choice between science and friendship. His regard for the feelings and reputation of his friend triumphed over his respect and recognition of scientific fact. This episode very clearly reveals the important role that science sometimes played in the private lives of the eighteenth-century virtuosi.

Hindered from naming the Cape jasmine after the man who was the proper recipient of the honor, Ellis began searching for a suitable name, one worthy of so handsome a plant. He considered it, he said, "the most rare and beautiful shrub, that has yet been introduced into the European gardens;" some of its double milk-white flowers had a width of four inches; it bid fair to reach a height of six or seven feet. Surely "Augusta" perfectly described such a plant. Ellis wrote to Linnaeus:

> I must therefore desire you would call this plant Augusta, which I think as well deserves that title for its elegance in every respect, as the Methonica does to be called Gloriosa. This will not offend our friend Warner's modesty, nor his particular delicacy to Mr. Miller.

Three times Ellis urged the adoption of this name; then he suggested an alternative:

> Be so kind as to call it Portlandia, after that eminent patroness of botany and natural history the Duchess of Portland, who is a very great admirer of your excellent and learned works, by which you have opened the eyes and understandings of mankind.

Which name Linnaeus preferred Ellis had no way of knowing, for the master made no comment.

In 1759 the second volume of the *Systema* appeared, but it contained no reference to Ellis' plant. The feelings of the virtuoso were deeply injured. He made no attempt to conceal his disappointment from his friends in London, nor did he address any letters to Linnaeus between March 1759 and June of the following year.

Ellis harbored no resentment against Linnaeus, however, for failing to name his jasmine after the Duchess of Portland. The alternative name had reached Linnaeus too late. In the tenth edition, which had just come to hand, Ellis observed that the *Portlandia* was listed among the new genera.

At length news of his sulking disciple reached the master at Upsala, and he wrote:

> [Professor Ferner] informs me that you are displeased at my not having admitted your new genus, by the name of Augusta, in the second volume of my Systema. Allow me to state my reasons.
>
> . . . I have laid down a rule in my Critica and Philosophia, that no adjective should be admitted as a generic name. On this ground I have expunged several names of other authors; but, that I might not carry innovation too far, I admitted Mirabilis and Gloriosa, for which I have often been blamed by my adversaries. Everyone knows that the Harlem florists give this kind of names to their Hyacinths, Tulips, &c. such as superba, augusta, incomparabilis, pulcherrima.

Ellis' recovery from his fit of the vapors was almost instantaneous. "What you say is right," he answered. Indeed, Ellis might almost feel that he owed a debt of gratitude to Linnaeus. Had not the master saved Ellis' "elegant plant" from the stigma of the market place?

Because Linnaeus had urged him to submit a name so that a description of his plant could appear in the appendix to the forthcoming Volume II of the *Systema*, Ellis was forced to act without delay. A glance at the second volume informed him that Linnaeus had ignored the suggestion, made in 1757, to honor Garden with the plant often called "Buereria." Instead, Linnaeus assigned the name *Calycanthus* to this new genus.

Ellis was not slow in making his decision. "I shall write to Dr. Garden this day," he announced to Linnaeus, "that I have desired you to give the name of Gardenia to the Jasmine, which I am persuaded he will esteem as a favour." And observe Garden's reply: "Your compliment of the Gardenia was most acceptable to me . . . I shall gratefully remember it. Has Linnaeus adopted it?"

Linnaeus' gardenia. There was the rub. Linnaeus was showing himself most reluctant to adopt the name suggested by Ellis. He warned his disciple

that if this request was granted his carping critics might renew their attack.

> If I may without reserve lay open my mind to you, I would have wished that the supposed Jasmine might have been called Warneria, after the person who has first cultivated it in Europe; Gardenia being applied to some genus first discovered by Dr. Garden. I wish to guard against the ill-natured objections, often made against me, that I name plants after my friends. . . . If therefore I confer this honour on those who have discovered the respective plants, no objections can arise, nor can I be charged with infringing my own rules.

This time, however, Ellis did not reply as he previously had done: "What you say is right . . . and I therefore submit." On the contrary, he maintained a strict silence.

Linnaeus wrote a second letter to London, and from it Ellis could infer that his request seriously interfered with Linnaeus' own plans to honor his disciple in Carolina and at the same time oblige his good friend in London. "I had given the name of Gardenia," wrote Linnaeus, "to an entirely new and very singular genus . . . in order so far to conform to your wishes."

Nevertheless, Linnaeus must have concluded that the bonds of friendship were stronger than the consistencies of scientific method, or even a master scientist's prerogative, for on November 4, 1760, he wrote to Ellis:

> As you still persist in your decision, that the Jasmine so often mentioned between us should be called Gardenia, I will comply, though I cannot but foresee that this measure will be exposed to much censure. I find it impossible to deny you anything. All that I beg of you, my dear friend, is, that you would publish the genus and its character in some loose sheet, or some periodical work, or transactions; in which case I promise to adopt the name. . . . Do not therefore indulge any more suspicions of my regard and devotion to you, who esteem you among the chief of my friends.

Ellis forthwith composed his description and obtained Ehret's handsome drawings; both appeared in the *Philosophical Transactions* of that same year.

At long last Alexander Garden had been presented with a "name-sake," and his fame was assured when the *Gardenia* took its proper place in the *Species Plantarum* of 1762. Thus the story of the gardenia explains why only Ellis' Cape jasmine had the honor of inclusion in the twelfth edition of the *Systema* (1767) even though five "gardenias" were entered in competition for that prize.

CERTAINLY if an eighteenth-century man asked, "What's in a name?" he would get no dusty answer from the botanical virtuosi. The story of the gardenia indicates that the giving of names to plants was a far-from-simple task. The popular identification of the plant with the man it honored caused the amateur scientists of the period to attribute a crucial social importance to the naming of plant genera.

One of the six plants that Garden sent to Ellis soon after their correspondence began was the "Carolina Yellow Jessamy." Although Linnaeus had already classified this plant, Garden believed it to be "absolutely a new genus." He wished to make his new friend a gift of the plant by naming it "Ellisiana." Apparently, Ellis advised against challenging the authority of Linnaeus, and so in a subsequent letter Garden wrote:

> I now have a plant which is entirely new, and the most superb lofty plant that ever I met with in America, which I shall beg leave of you to accept as a name-sake . . . but if you should chuse a shrub, I have that beautiful one which I call the hop-like shrub.

Once correspondence with Linnaeus was established, Garden renewed his efforts to compliment the man to whom he once referred as "my dear, my first, my chief Botanical friend." In 1758 Garden submitted the characters of "a very handsome plant," which, he told Linnaeus, "I am very anxious should bear the name of my much valued friend Mr. Ellis." Linnaeus rejected the plant, and he further dashed Garden's hopes by stating that Ellis already had been presented with a plant. In a grateful but saddened mood Ellis wrote to Garden, "Indeed your compliment of the Ellisia (now a Swertia) I can never forget. You see Linnaeus says I must be contented with one place, which Dr. Browne has given me."

In 1762, however, Linnaeus sent Ellis startling news concerning his plant:

> I . . . am obliged to confess that Duranta and Ellisia not only form one genus, but scarcely differ in species, so that the latter cannot be kept distinct. This being the case, I began to look about for a new Ellisia, that you, who deserve so eminently of our science may not be forgotten. Being particularly desirous to fix on some plant known in the gardens of Europe, I have thought of . . . Ipomaea Nyctelea. . . . Write to me as soon as possible, whether this meets your wishes, or whether you would prefer any thing else.

Ellis did indeed prefer something else. He knew Linnaeus' plant well; he had even made a drawing of it for his friend Peter Collinson. What Ellis desired for his "name-sake" was the plant that Garden had sent Linnaeus in 1760. The Charleston physician had anticipated it would be named after him, but its arrival coincided with the moment when Linnaeus had agreed to have Ellis' Cape jasmine called a gardenia. Ellis had suggested at that time that the plant be named "Schlosseria" in honor of the Dutch botanist.

As soon as possible Ellis wrote Linnaeus, reminding him of the plant "with white flowers" that Garden had sent to Sweden:

> If it is not too late, and you find it a distinct genus, I would rather choose to change plants with Dr. Schlosser. If this is not convenient, and you have any new genus of a specious plant, which will grow in England, you will do me much honour; because I may communicate it to the gardens of my friends here, to put them in mind of me.

Ellis had no wish that his friends should identify him with the plant which Linnaeus had selected:

> You will pardon me when I tell you that people here look on a little mean-looking plant as reflecting no honour on the person whose name is given to it, though I am convinced, as it is a distinct genus, the compliment is equally great with the greatest tree.

Ellis might reason as a scientist, but he shared the feelings of the virtuosi.

There is no evidence that Linnaeus was impatient with the unscientific sentiment expressed by Ellis; perhaps he was aware that his amateurs attached social as well as botanical importance to the naming of plant genera. I am inclined to believe that Linnaeus would have given Ellis his "very pretty shrub" if it had not already been named. Ultimately, Ellis was willing to accept with good grace Linnaeus' gift of the "mean-looking plant" with its tiny blossoms and to suffer in silence the inevitable invidious comparisons.

Consider, in contrast, Alexander Garden's pride and gratification in the plant that his London friend and his master at Upsala had bestowed upon him. It had all the virtues: first of all, it was large; its flowers were huge and showy; it was blessed with perpetual verdure; it had a "refreshing aromatic smell." News reached Garden from Ellis that gardenias were in such popular demand in London that James Gordon could charge five guineas for each plant. The gardener at Mile End enjoyed a local fame, and over a period of three years he became the richer by five hundred pounds.

Garden must have been aware that he was a very lucky man. What but luck could explain why Linnaeus did not give Garden's name to any of the other proposed plants? Why had the crucial date of 1760 passed before Linnaeus began adopting the new plants Garden sent to him? There was precedent for giving the so-called jasmine such a name as "Augusta," but fortunately Linnaeus had refused Ellis' request; had Ellis submitted the alternative name only a short time earlier, the plant might have been a compliment to the Duchess of Portland. To all these happy coincidences must be added Garden's extraordinary good luck that circumstances prevented the plant from receiving its proper name of "Warneria."

I doubt if the hapless Mr. Warner was considered either misguided or foolish by the virtuosi of his acquaintance. That was a sentimental age, and Warner was indeed a man of sentiment. Out of consideration for the tender feelings of his friend he forfeited the fame that this large and handsome plant could bring him. As time passed and the popularity of the gardenia increased, his fellows might well have viewed Warner as almost a pathetic hero. The attitude of Philip Miller would also be comprehensible to the virtuosi. Is it likely that Miller's *amour-propre* would have suffered any serious injury if Warner had publicly challenged his friend's faulty judgment about a "little mean-looking plant"?

Meanwhile Garden's personal triumph was real and sweet. His name was well publicized in *Philosophical Transactions*; his plant was assured a life beyond life in the *Systema*. A letter from London in 1761 gave the impression that the gardenia was the talk of the town. Ellis exclaimed: "Every body is in love with it . . . It has given great jealousy to our botanists here, that I have preferred you to them; but I laugh at them and know I am right . . ."

So secure was the fame of the gardenia in 1763 that Garden could afford to jest over the "sudden death" of the plant growing in his garden at Charleston. "I take it to be no good omen," he said, "for the continuance and duration of my botanical name and character." Then he added, "But if I do not outlive it, I shall be pleased."

Even beyond his lifetime Garden continued to be fortune's favorite. The plant that now bore his name was called Cape jasmine by Garden's contemporaries, and it is natural that the public did not discontinue at once the use of a name so attractive as this. In time, however, after the new name had replaced the old, the gardenia and the man whom it honored became forever linked in the minds of men. It is a pity Garden did not have the pleasure of knowing that almost two hundred years later it could still be said of the gardenia, "Everybody is in love with it."

ARE OUR WARS GOOD TIMES?

WILLIAM F. OGBURN and JEAN L. ADAMS

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THE effect of war is generally considered to be destruction. Certainly, such it is in battle and in a country that is bombed or invaded. Otherwise, the destruction is limited to materials and lives lost outside the home country. But there are also constructive effects of war, as will be noted in this article.

First, we observe that the consequences of war are much more than loss of life and property. Industry, agriculture, government, and schools are affected, for illustration. In modern wars there is hardly a social institution that escapes its influence, and this influence is not always bad. The purpose of this article, however, is not to be so comprehensive as to survey the influence of war on the various social institutions. We wish rather to compare the conditions of recent wars in the United States with the conditions of the business cycle in peacetime.

For such comparison, we shall be restricted to statistical records, such as those of production, unemployment, divorces, and births. By observing the movement of curves from such annual statistical records in peace and in war years, we shall make some deductions regarding the influence of war. For instance, the annual number of failures of industrial and commercial firms per 10,000 firms in the 1920s and 1930s was 90. In the war year 1944, the number was only 7 per 10,000 firms. We therefore conclude that one influence of the war upon business in the United States was to reduce greatly the number of business failures.

War years not easily discernible in some records. Not all social conditions are so markedly affected by war as are business failures. The hours worked per week in wartime, for instance, are not greatly different from the number worked during peace. In the war years of 1942–44 the average number of hours worked per week in the manufacturing industries in the United States was 44; in the depression years of the 1930s the average was 38 hours. But in the prosperous years of the 1920s, from 1923 to 1929, the weekly working time in factories was 44.6 hours, or slightly more than in World War II. Whatever the influence of the war on hours worked per week, it was not profound.

For some statistical records during peace and war years it is difficult to tell from the curves alone which section of the curve is in the war period. This difficulty is illustrated by Figure 1, which sets forth undated curves of six annual statistical series. Each of these curves is for a twenty-year period, but not for the same twenty years. Each curve, however, does cover the period of either World War I or World War II. Looking at this undated chart, the reader will not find it easy, on the basis of the fluctuations of the curves alone, to locate the war years on each curve of the chart. In the series depicted in Figure 1, what occurred in war years could have happened in peacetime, and vice versa.

For curves of other statistical series, the war period may be recognized approximately. Yet in such curves, undated, it is difficult to determine from them alone exactly when the war began. Six such curves are shown in Figure 2. Each of these curves is for a different seven-year period covering part or all of the second world war. The reader will be interested in trying to find from the curves in this chart the exact year that the United States entered the war. Although the bombing of the fleet at Pearl Harbor came with suddenness, the day is not so dramatically registered on these time-series. Most of the undated curves in Figures 1 and 2 are found dated in Figures 3, 4, or 5.

Figures 1 and 2 indicate that in some statistical records the recent wars of the United States have not produced enough of a change to be easily recognizable, or the change produced has not been very greatly different from what has occurred in peacetime.

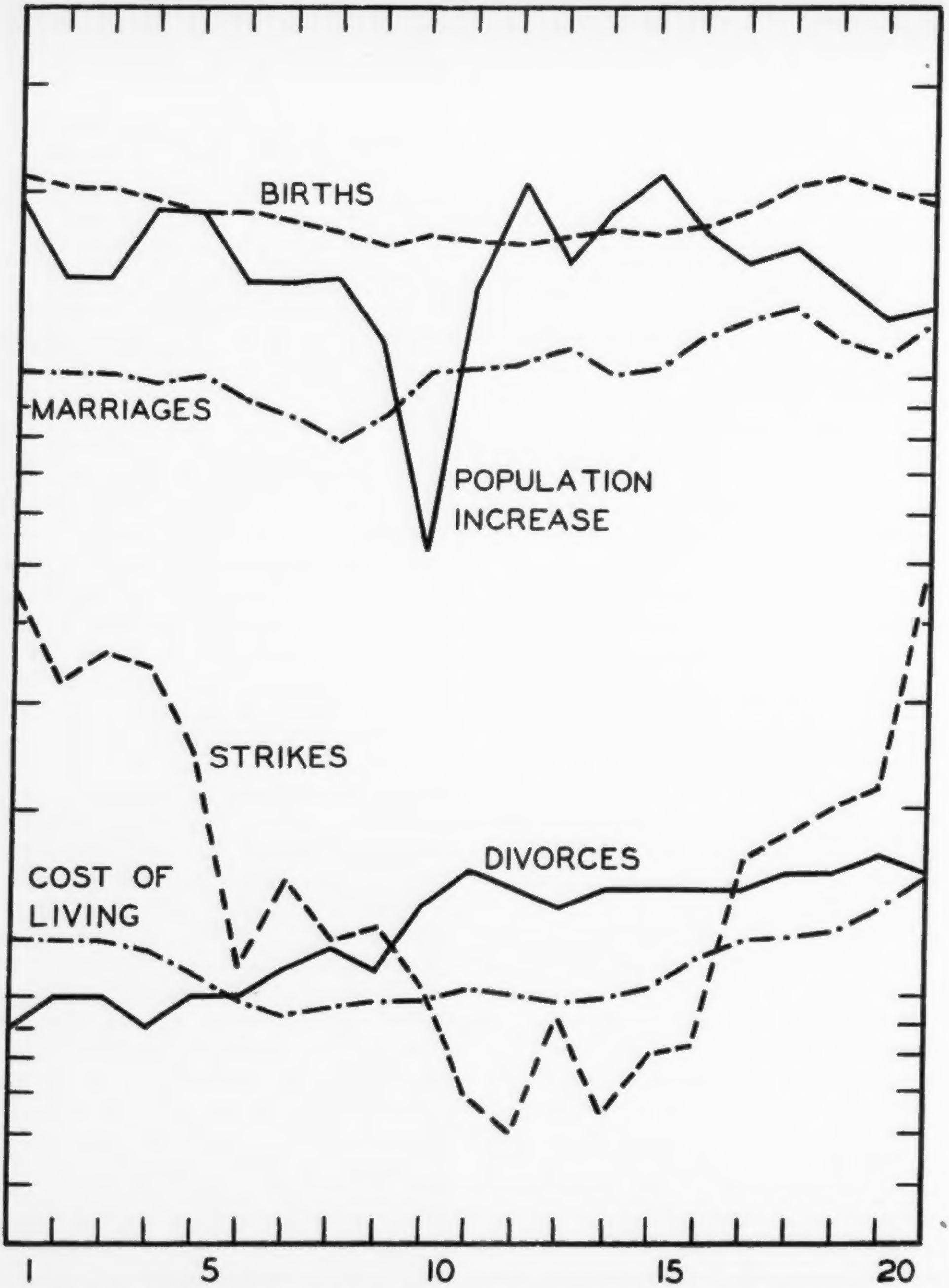


FIG. 1. WAR YEARS AND PEACE YEARS ALMOST INDISTINGUISHABLE

The six records cover 20-year periods, but the periods, dates given in the following paragraphs, are not the same; hence, the figures on the base line of the chart are not dates but numbers of years. Each series includes the years of either World War I or World War II. Without knowing the dates, it is rather difficult to locate the war years on the curves. The variations in the war years, then, are not significantly different from the variations that might be expected in peacetime.

Strikes and Lockouts (1917–37), total number of stoppages in thousands; Marriages (1925–45), rate per 1,000 population; Divorces (1910–30), rate per 1,000 population; Births (1925–45), rate per 1,000 population; Population Increase (1910–30), in millions, calculated from estimates of Bureau of the Census; Cost of Living (1927–47), index number of the Bureau of Labor Statistics; all series taken from *Statistical Abstract of the United States.*

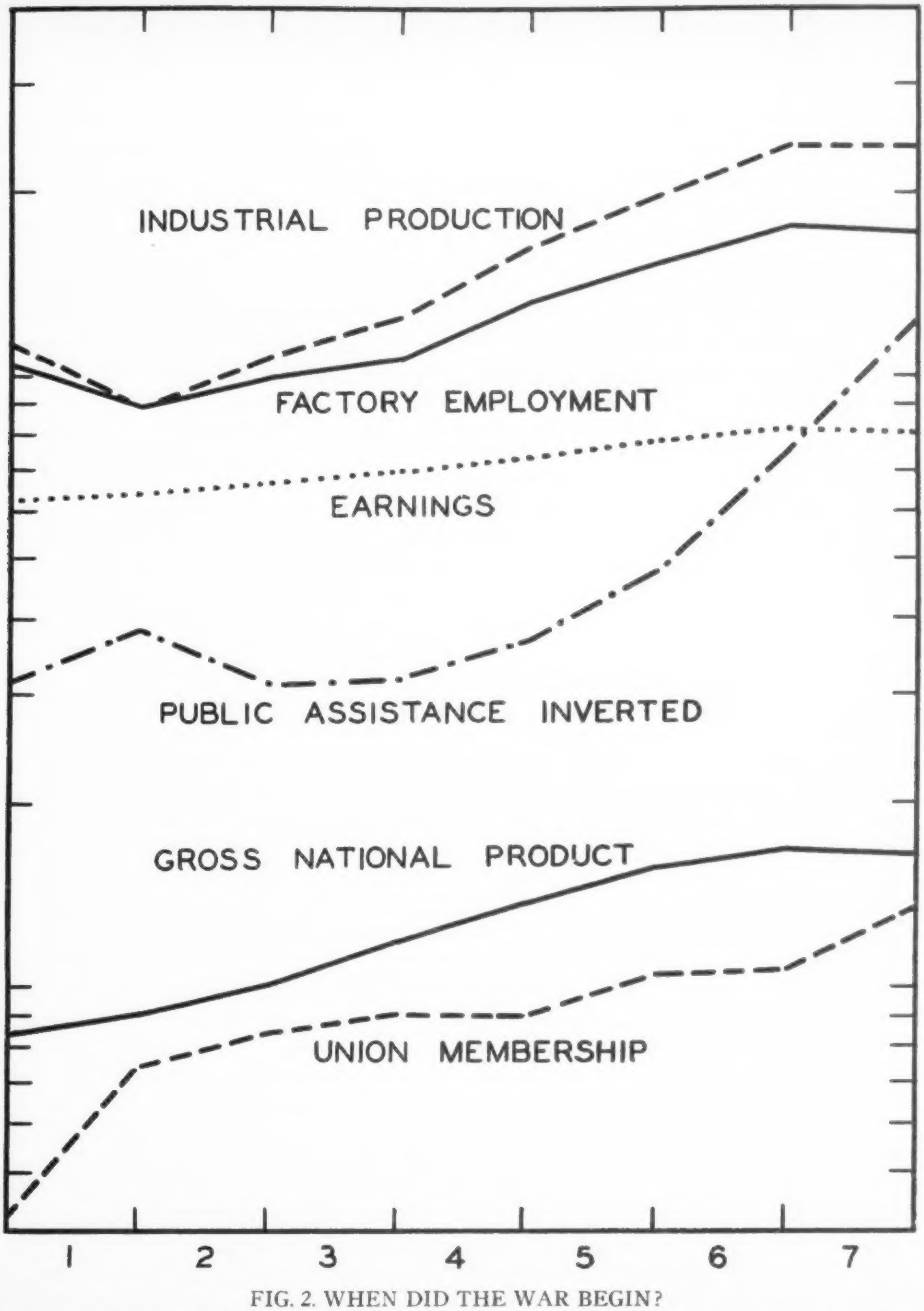


FIG. 2. WHEN DID THE WAR BEGIN?

The six curves include part or all the war years of World War II, but all curves do not cover the same seven-year period. We cannot tell from these undated curves, though, exactly when the war began.

Labor Union Membership (1936–43), in millions of members, from *Labor Information Bulletin,* August 1947, p. 4; Industrial Production (1937–44), index number from *Federal Reserve Bulletins,* 1935–39 = 100; Real Hourly Earnings of Production Workers in Manufacturing (1938–45), cents per hour, deflated by Cost of Living Index, from *Statistical Abstracts*; Factory Employment (1937–44), index number, *Federal Reserve Bulletins,* 1939 = 100; Gross National Product (1938–45), in billions, deflated by Cost of Living Index, from U. S. Department of Commerce, *National Income,* Supplement to Survey of Current Business, July 1947, p. 19; Public Assistance (1936–43), inverted, millions of dollars, deflated by Cost of Living Index, Federal Security Agency, Social Security Administration, *Social Security Yearbooks.*

The indices of prosperity measured our war years. Statistical series, presented like those in Figures 1 and 2, are used by economists to represent the fluctuations of business conditions. These fluctuations alternate between prosperity and depression. Such a course of business is often called a business cycle, although the alternations of business are not as smooth as the cycle of the tides. Five statistical series are commonly used to describe the course of business. These are industrial production; bank clearings outside New York City; wholesale commodity prices; freight car tonnage, representing production, trade, marketing, and transportation; and the percentage of all firms failing.

These five curves are shown in Figure 3. The depression years of the 1930s are clearly indicated, as is the prosperity of 1919–20 and the late 1920s. But the curves that are high in the prosperous business years of peacetime are also high in the years of World Wars I and II. This observation, we think, is important. The curves, used to show the business cycle, have the same behavior during the two world wars as during periods of peacetime prosperity. This behavior suggests that business was prosperous during war periods. This startling statistical inference is in sharp contrast to the idea that war consists only of horror and destruction, which it did in many countries.

The observation that war was a period of prosperity in the United States, as indicated by the time-series in Figure 3, may be explained by those to whom the idea is unwelcome by stating that the curves in Figure 3 (except the one on business failures) show only business activity, and not necessarily prosperity. Prosperity, it might be argued, is shown by profits; and that, although profits normally go with large production in peacetime, such need not be the case in wartime. In other words, industry could be very active in war years, from patriotic motives only, without making any profit at all. Such might be the case, but the curve of profits in Figure 5 shows that profits, like production, were greater than usual in the war year 1917, and again in the years of the second world war, 1942–45. Since the number of plants in operation was only a few more in 1917 than in pre- and postwar years, and was fewer than usual in 1942–45, we infer that the profits per firm were higher than normal.

The curves, then, which are used to measure the business cycle make the war years in the United States look a great deal like the prosperity phase of the business cycle.

Good times accompanied our war years. The years when business is prosperous are called "good times," that is, good times in general, not merely good times for business. There is more money to spend for clothing, for recreation, for travel, and for food. Institutions other than business—for instance, the family and the church—experience good times, too, when business is prosperous. There are more marriages, more births, a better standard of living for farmers, fewer suicides, fewer murders, fewer admissions to prisons, an increase in the membership of labor unions, less public assistance for the needy, and a higher income for the nation.

There are, though, some conditions of life in periods of prosperity that might not be properly called good times, but rather bad times. For example, there are more divorces granted in years of prosperity than in years of depression; and, strangely, there are more deaths in so-called good times than in bad. That we call periods of business prosperity good times rather than bad times indicates that there are more desirable than undesirable conditions accompanying prosperity.

It is interesting to see how the thirteen indices of good times listed in a preceding paragraph fare during wars. This has been done in Figures 4 and 5. These indices indicate both world wars as good times as measured by these thirteen series. In wartime, there was more marrying, fewer murders, more money to spend or save, etc. The associations of the war years were, then, in many activities of life in the United States, pleasant ones.

Distressing conditions found in war years, too. That there were unpleasant associations with wartime is also true. There were fear, sorrow, and separation from loved ones. Unfortunately, there are no index numbers for fear and sorrow; though the 14 million in the armed services is an indication of the extent of separated families and lovers. From the reservoir of time-series in the statistical yearbooks, a few annual series representing regrettable conditions are found (Fig. 6).

Looking at these curves, we may reflect on the degrees of distress they indicate for the war years. The curve for highway construction was low during the war, and discomfort was caused by the bad state of repair of the roads of our states and the streets of our cities. Taxes were higher during the wars, but increments of national income added were larger than the increments of taxes. The vast accumulation of the war debt was frightening, but its burden will be felt more in the years following the war than it was during the war. Rather, during the war the debt added an inflationary buoyancy.

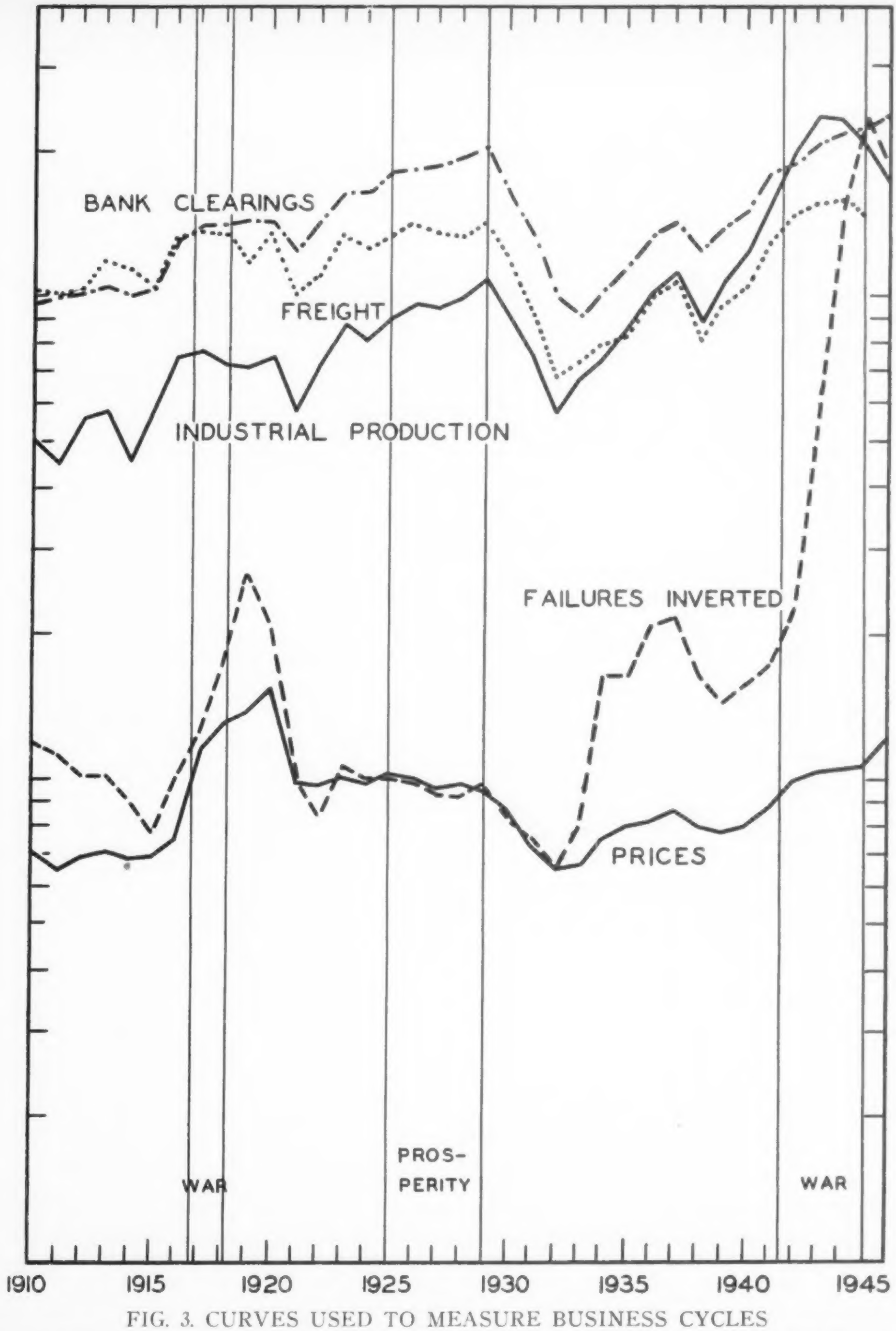


FIG. 3. CURVES USED TO MEASURE BUSINESS CYCLES

The curves ordinarily used to measure business cycles when traced through the period of World War I and World War II show the war years to be like the prosperity phase of the business cycle, as seen in the years of the late 1920s. If the war years were unknown, the curves alone would seem to indicate these war years merely as periods of business prosperity, which are higher on the chart than the preceding and following depression phases.

Freight, billions of tons of revenue freight originated, from *Statistical Abstracts*; Commodity Prices, Wholesale, index number from *Fed. Res. Bull.*, 1926 = 100, unadjusted; Industrial Production, index number from *Fed. Res. Bull.*, 1935–39 = 100; Bank Clearings Outside New York, in billions of dollars, deflated by Cost of Living Index, from *Com. and Fin. Chron.;* Fairures, industrial and commercial, inverted, number per 10,000 enterprises, from *Statistical Abstr.*

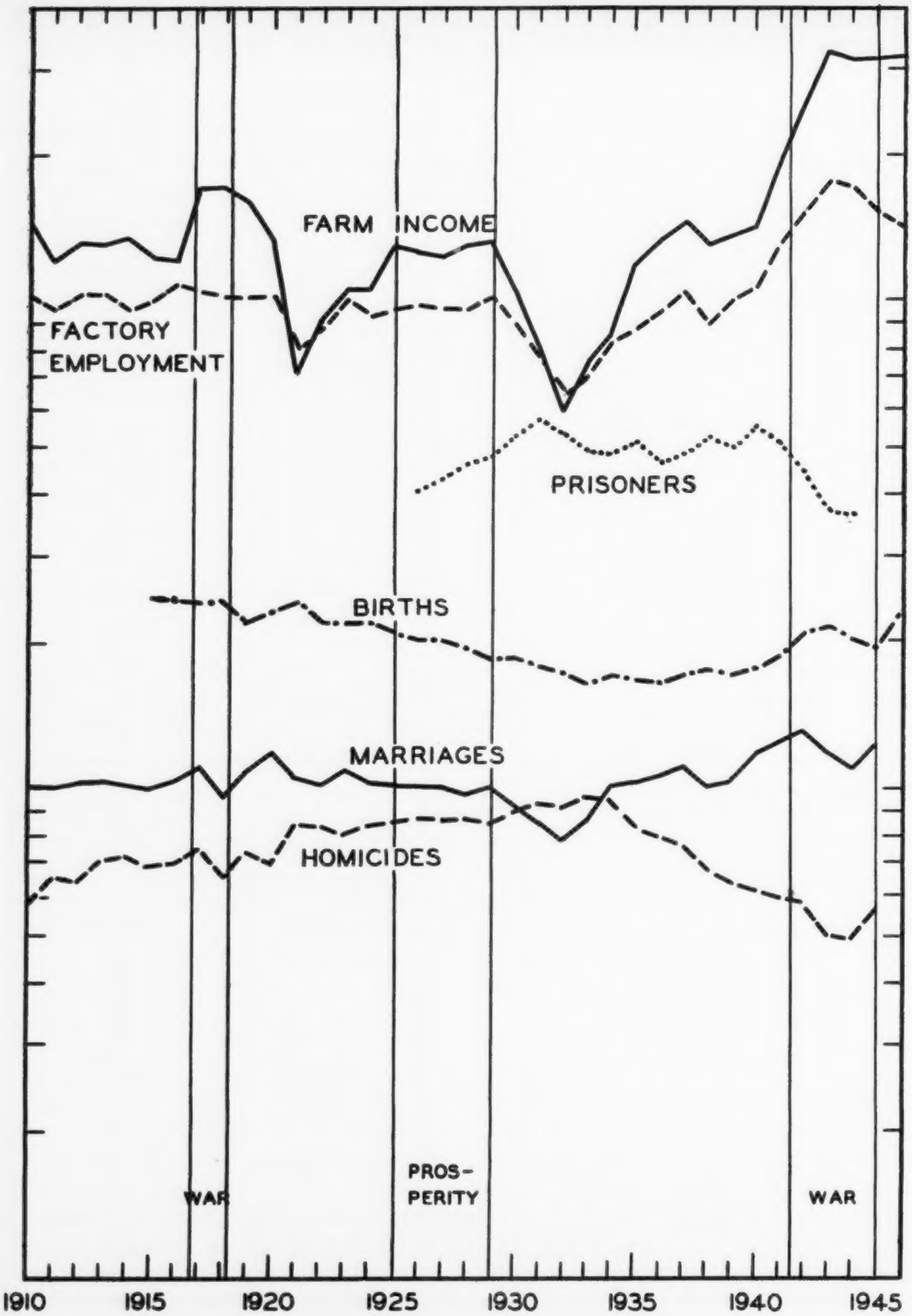


FIG. 4. GOOD TIMES IN WAR YEARS

With business prosperity are associated good times in other areas of life's activities. In Figures 4 and 5 curves are drawn that have been found correlated with the business cycle and are used as evidence of good times. These indices of good times generally show the same characteristics in war years as in periods of prosperity in peacetime; that is, the curves are higher (or lower) in both war years and years of prosperity than they are in periods of business depression.

Marriages, rate per 1,000 population; Births, rate per 1,000 population, both from *Statistical Abstracts*; Factory Employment, index number, *Federal Reserve Bulletins,* 1939 = 100; Farm Income, net income from farming to all persons on farms, per capita in hundreds of dollars, deflated by prices paid by farmers, including interest and taxes, 1910–14 = 100; Homicide Deaths, rate per 100,000 population, from *Statistical Abstracts*; Prisoners Received from Courts during the year, Federal and state prisons and reformatories, rate per 100,000 population, from *Statistical Abstract of the United States.*

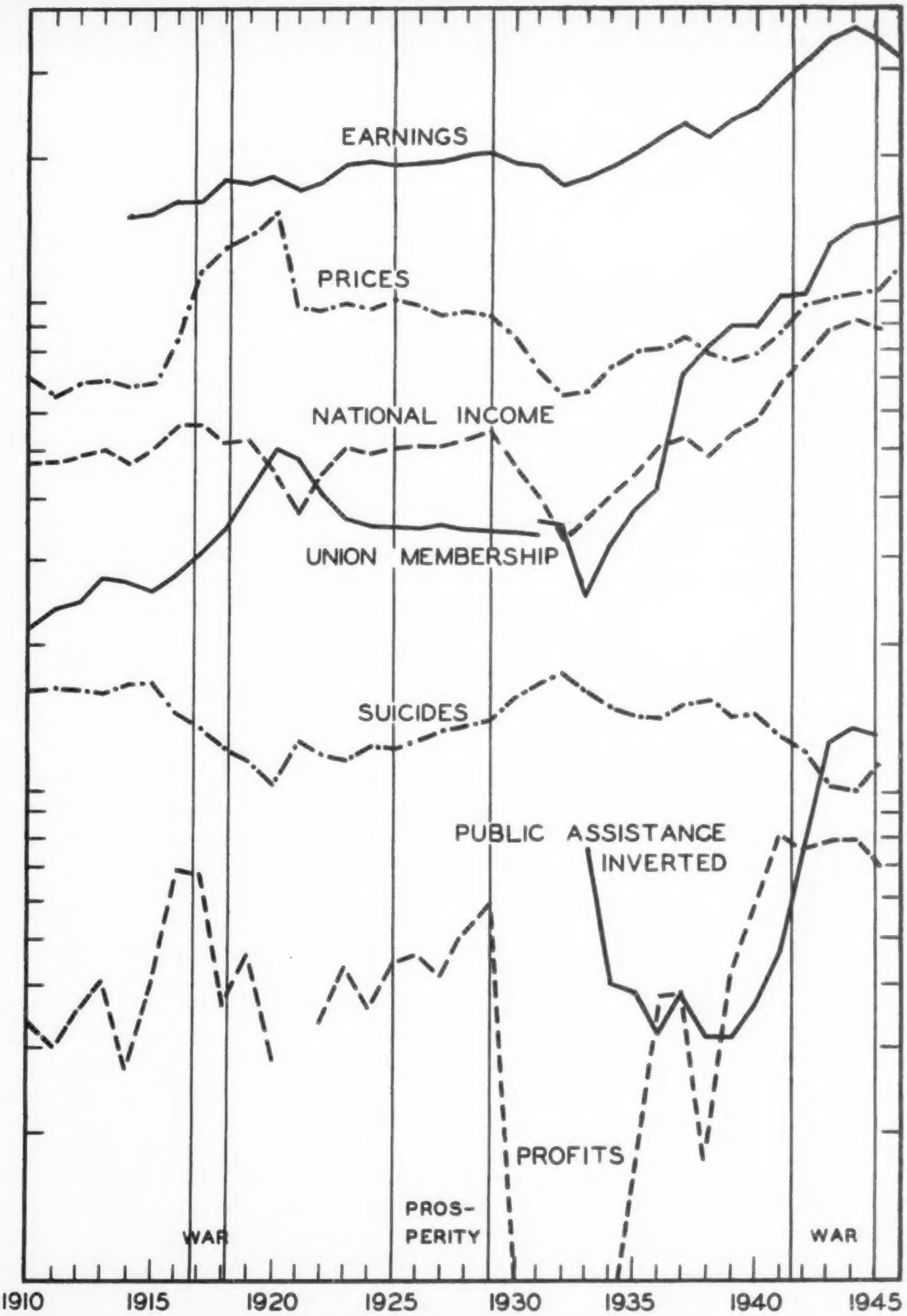


FIG. 5. GOOD TIMES IN WAR YEARS (*CONTINUED*)

Weekly Earnings of Production Workers in Manufacturing, deflated by Cost of Living Index, in dollars, from *Statistical Abstracts*; Commodity Prices, Wholesale, index number from *Federal Reserve Bulletins*, 1926 = 100, unadjusted; Suicides, rate per 100,000 population, from *Statistical Abstracts*; Labor Union Membership, in millions of members, 1910–1931 from Leo Wolman, *Ebb and Flow in Trade Unionism*, p. 16, average annual membership, 1931–1946, from *Labor Information Bulletin*, August 1947, estimated from graph; Public Assistance, inverted, millions of dollars, deflated by Cost of Living Index, from Federal Security Agency, Social Security Administration, *Social Security Yearbooks*; Net Corporate Profits, in billions of dollars, deflated by Cost of Living Index, below the bottom line of the graph is a loss except for 1934, which had a profit of .5 billion dollars, but is not shown—no figure is available for 1921—from *Economic Almanac*, 1946–1947, p. 44; National Income, in hundreds of dollars per capita, deflated by Cost of Living Index, from *Economic Almanac*, 1946–1947, National Industrial Conference Board figures.

It has been claimed that a smaller percentage of the eligible population voted in local elections during war years and that there was less interest in local affairs, but there are no published statistics available.

Divorces and deaths are tragic, but their wartime increase actually touches only a small proportion of the families. Divorces numbered 1,400,000 during the second world war, but without war there would have been about 940,000 divorces, assuming the same divorce rate as from 1937 to 1939. Thus, the added contribution to the number of divorces during the second world war was 460,000, whereas the number of married couples was 32 million.

The war deaths were about 350,000; in three and a half years of peace at that time there would have been around 5 million. Thus, the war added 7 percent to the number of deaths. Of course, no one of the families that furnished 14 million men to the armed forces knew that death would not strike. So there was fear, the memory of which remains as a terrible association of wartime, for which there are no statistics. To these distressing associations with war should be added the hard and fearful conditions of life for men at the front. One third of the medical discharges during World War II were for neuropsychiatric reasons.

We do not think it possible to strike a net balance between the good and the bad associations of war years in the United States. To many, the utter horror of war overshadows all else. Nevertheless, the data of this article do show that the indices we use to measure good times characterize the periods of our activity in the past two world wars. These observations are made on the United States and not on France, Japan, or Russia, where the sufferings of war were much more evident.

War and preparation for war. Our analysis has concerned the movement of certain indices of social conditions during war years, but in the United States the years of war were not all spent in fighting. During the first parts of World War I and World War II we did little or no actual fighting, for we were not prepared to fight. We entered both world wars suddenly, with our soldiers inadequately trained and equipped. So the war periods were divided into two parts. The first part was characterized by preparation, which might have been done in peace years; in the second part we were engaged in actual fighting. We were able to be at war without much fighting because our allies held back the enemy.

The question arises, then, as to whether the prosperity and good times that characterized the war years of the United States were due to the preparation for war or to the conditions of a fighting war.

It is easy to see how preparation for war would bring great business activity and profits; and, of course, the good times that accompany industrial prosperity. But how about the fighting years of war after the initial period of preparation was over? In order to compare these two parts of our war years we have prepared Figure 7, in which are drawn some statistical indices of business conditions by months during the period between the declaration of war and the collapse of the enemy. The first year of this period and most of the second year were months of preparation, with little fighting. During the last year and a half of the war period, the participation of armed forces in battles and bombings increased.

The curves in Figure 7 were rising in the first part of the war, generally, until around the close of 1943 or the beginning of 1944, as they do in the business cycle with the recovery of industry from a depression. Such was expected. The question is, How did these curves behave after the period of preparation was over? The chart shows that they generally ceased rising, and flattened out. In no case was there a significant recession during the fighting years of World War II, from the peak or plateau of business prosperity. In other words, the short period of the actual fighting war was characterized by the same industrial boom times that signalized the period of preparation for fighting. We do not know what the course of the curves in Figure 7 would have been if the war had lasted three or four years longer, nor what it would have been if we had been invaded or bombed extensively.

Conclusion and discussion. The conclusion of this study as shown by the data is that the experiences at home in the United States of our social and economic institutions and activities during the periods of our participation in the last two world wars were much like those of the prosperity phase of the business cycle in peace years, commonly called good times; though there were some incalculable conditions of fear and distress.

The United States was more fortunate than were the combat countries of Europe and Asia that were bombed or invaded and that were in the war longer. Our industries were more prosperous than in other warring countries. The positive association of good times in the United States during the war probably has a bearing upon our willingness to engage in another war. Rewards and punishments, we know from psychology and from experience,

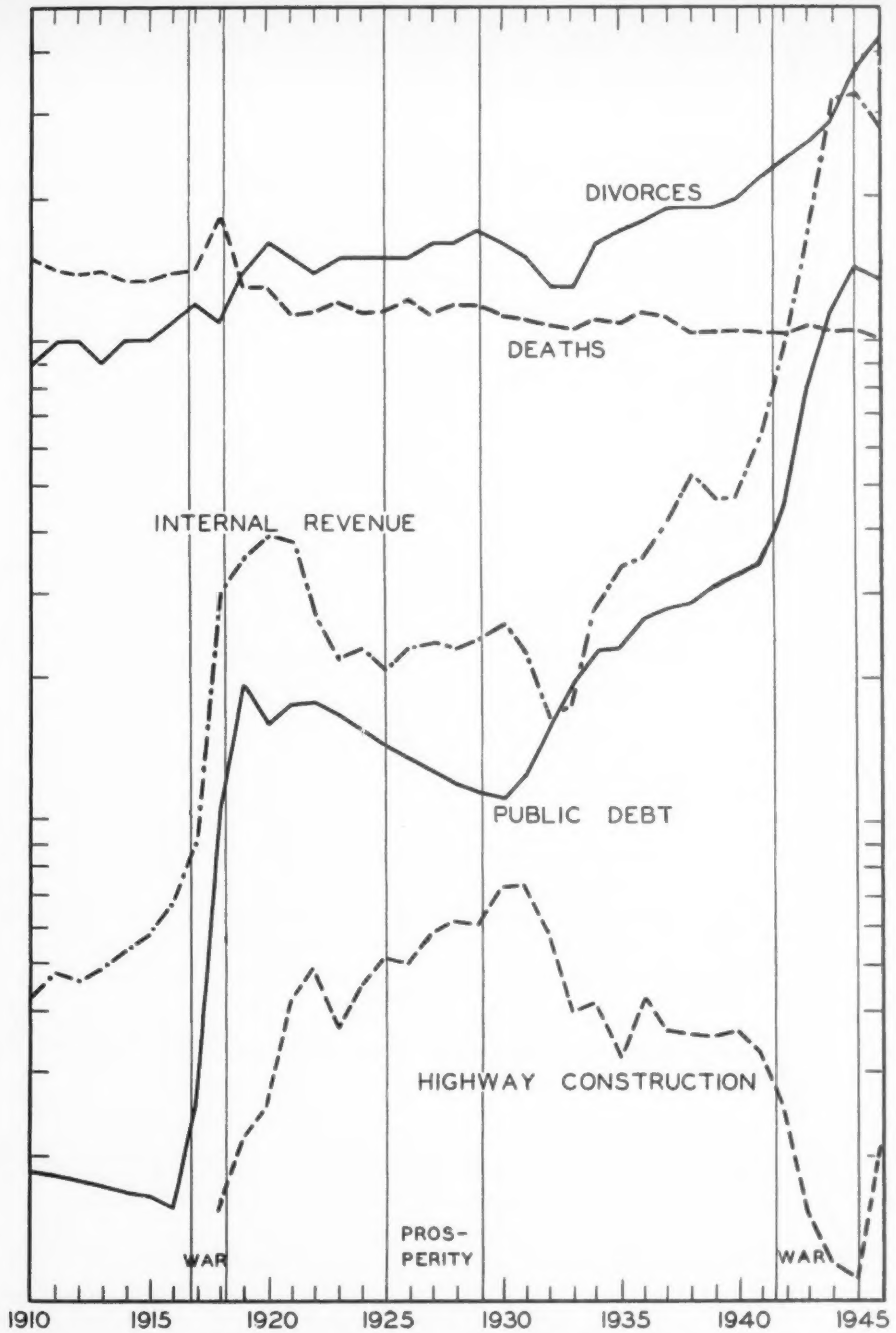


FIG. 6. SOME DISTRESSFUL CONDITIONS OF WAR YEARS

Not all the activities of war years can be called prosperity and good times. Statistical records reveal some unfortunate conditions, such as those shown by the five curves of time-series found in the statistical yearbooks. There are other distressing conditions in war years, such as fear, for which there are no statistical records.

Deaths, rate per 1,000 population; Divorces, rate per 1,000 population; Public Debt, dollars per capita, deflated by Cost of Living Index; Internal Revenue, income and profits taxes plus other revenue, in billions of dollars, deflated by Cost of Living Index, all from *Statistical Abstracts*; Highway Construction, in millions of dollars, 1918–1929, from *Economic Almanac*, 1946–1947, p. 235, 1929–1946 from *Statistical Abstr.*, 1947, p. 968, deflated by *Eng. News Record* construction cost index, which measures the movement of construction costs in general, from *Statistical Abstr.*, 1947, 778.

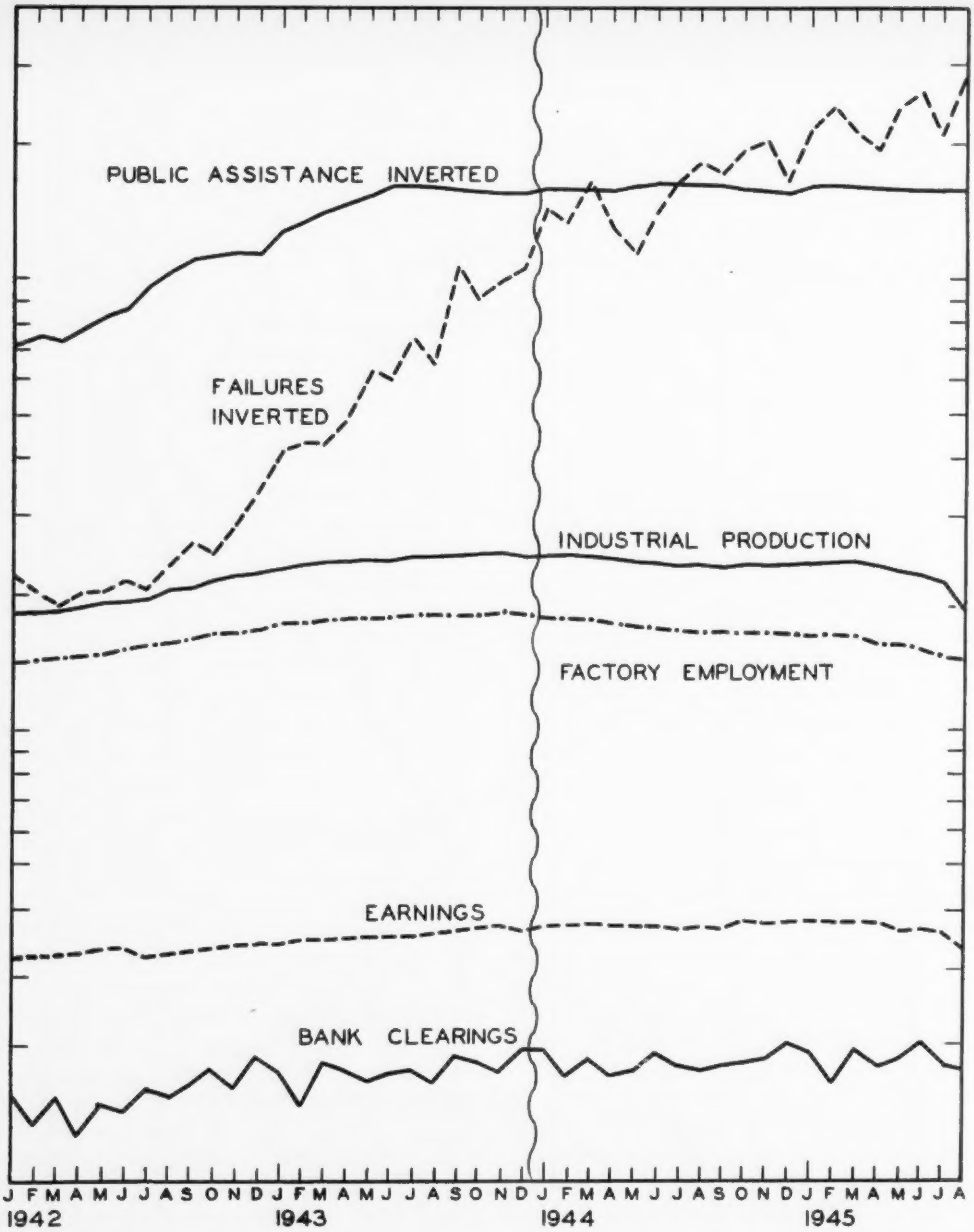


FIG. 7. PREPARATION FOR WAR, AND COMBAT WAR

In the first part of the years in which the United States was officially at war in 1941–1945, activities were largely preparatory. In the latter part of the period, combat fighting was much more extensive. The wavy line indicates, approximately, the division between these two periods, but more accurately for some of the curves than others. These curves, which have been used to indicate good times and prosperity in peace years, are high both in the preparatory and in the fighting phases of the war years.

Industrial Production, index number from *Federal Reserve Bulletins*, 1935–39 = 100; Bank Clearings Outside New York, in billions of dollars, deflated by Cost of Living Index, from *The Commercial and Financial Chronicles*; Weekly Earnings of Production Workers in Manufacturing, deflated by Cost of Living Index, in dollars, from *Statistical Abstracts*; Public Assistance, inverted, millions of dollars, deflated by Cost of Living Index, from Federal Security Agency, Social Security Administration, *Social Security Yearbooks*; Failures, industrial and commercial, inverted, number per 10,000 enterprises, from *Statistical Abstracts*; Factory Employment, 1919–1946, index number, *Federal Reserve Bulletins*, 1939 = 100, 1910–1918 from Paul F. Brissenden, *Earnings of Factory Workers, 1899–1927*, p. 61, decreased by average percentage of difference in 1919–1920.

are effective determinants of behavior. Animal trainers find that their subjects repeat more readily when rewarded and that they desist more quickly when punished. Our religions have a heaven and a hell. Rewards and punishments, however, are not the only factors bearing upon behavior and shaping our attitudes toward war.

In view of this possible influence of our past fortunate war experiences upon our attitudes toward engaging in future wars, it is well to remember that we may not be so fortunate in another war. We may be in another world war from its beginning and not after the enemy is partly worn down by fighting with our allies. Nor are our allies likely to hold the enemy while we take a year or two to prepare. It is almost certain, because of the existence of long-range bombers of great speed, that we shall be bombed in the next war. The bombing will be very destructive if our enemy has the atom bomb. Nor is it clear that such a war will be brief.

REMNANTS

These are still left: a budding, wayside elf
That droops dust-covered; the moth a blinded troll;
Birds are shy sprites hurtling from hole to hole
In the shattered veil of Earth; the Earth itself,
Fear host to little things that dare to thrill
And glow only in midnight mystery;
The dwindling mammals slip away and flee
From all erect, who crush what is not still.
Man ever burgeons! He is the imp that mars
All that is beauty, all the ethereal
Magic, Earth makes of gleam and waterfall;
Nothing is safe but cloud and brooding stars.
God save these remnants whatever Man may do,
When ribs of cities totter against the blue.

ROBERT THOMAS MOORE

BURTON E. LIVINGSTON, 1875–1948

WARREN B. MACK
Department of Horticulture, The Pennsylvania State College.

MY FIRST meeting with Professor Burton E. Livingstôn took place in his laboratory at Johns Hopkins during the summer of 1927, in an interview he gave me in response to my inquiry as to whether he would accept me as a graduate student. I arrived at his laboratory a little after 11:00 A.M., the hour he had named, and I was concerned about infringing upon his lunch hour. But he had left word with his secretary to call him at his home when I arrived, and she assured me that he would come in at once and would not worry about lunch.

The interview lasted until well into the evening, and my protests about delaying his supper were smilingly disregarded. Several conversations of similar duration took place before plans for my studies were ready to go into effect in the fall of 1928; these interviews, I discovered, constituted a good part of my preliminary examinations, informally conducted. It was impressed upon me, however, that it was necessary to clear the formal requirements of the University—"hurdles," he called them—as decisively as possible.

Lectures and laboratory exercises in plant physiology I found to be equally informal. Professor Livingston announced several days after the official opening of the University that he would lecture from 11:00 to 12:00 on Mondays, Wednesdays, and Fridays; actually, the lectures might begin at any time up to 11:45 and continue to 1:30 or later. Students brought their lunches to the laboratory and worked there until Professor Livingston started from his office to the classroom, when we all filed in with him. After the lecture, the students returned to their desks in the laboratory and ate their lunches; Professor Livingston accompanied one or more students, discussing their work or any other topic that might come up. Usually, the conversation narrowed down until it centered upon one student, and it would continue for an indefinite period, sometimes until 9:00 in the evening or even later.

As little attention was paid to the calendar as to the clock. The Friday after Thanksgiving Day was part of the holiday, but the students came as usual, and in due course the Professor appeared too. The same thing might have happened during the Christmas recess also, but the annual meetings of the American Association for the Advancement of Science then claimed Professor Livingston's time and energies.

His lectures, though informal in presentation, were well organized, and they were characterized by many references to the persons who had contributed to knowledge under each topic, many of whom had worked in his laboratory. Methods of measurement and their invention and improvement were allotted a great deal of time, and always the ones who invented and improved were named and paid full tribute.

As to laboratory exercises, typewritten outlines of the classical experiments were issued to each student, but after he had completed a few general experiments, the student was urged to examine critically one or a limited number of the experiments in which he had a special interest. Some of the students, for example, spent several months on Askenasy's demonstration of the tensile strength of water enclosed in capillary tubes; their work discovered the precautions required to ensure a reasonable degree of reliability to this demonstration, which formerly was a hit-or-miss affair.

Aimless activity or lack of sustained interest was discouraged. The student who characteristically wrote verses about raindrops coursing down the laboratory windowpanes and forgot to make readings on his instruments was encouraged to pursue studies elsewhere, even though the verses, according to Professor Livingston, were fairly good.

The objective point of view was maintained at all times; the student was required to observe quantitatively and discover relations among events and conditions, but he met little enthusiasm for speculation or conjecture. He had to account for all his observations and conform with rigid logical procedure. Of the student who thought that his technique must be faulty because a decapitated plant did not develop positive pressure in his experiment on root pressure, Professor Livingston asked: "How do you know? All plants do not develop the same pressure. Maybe this one did not develop any."

The Laboratory of Plant Physiology was small, but was well planned and well equipped; shops with wood- and metal-working and glass-blowing equipment provided ample facilities for the me-

chanically adept student, who met every encouragement to devise apparatus for more accurate measurement of processes, or for more precise control of environmental conditions. Apparatus such as porous cup atmometers, to measure evaporation, and unglazed porcelain cones, to measure the water-supplying power of the soil, which were developed in these shops, were deliberately designed to be simple and inexpensive, to bring them within the reach of as many laboratories and students as possible. For any purpose, however, the apparatus had to be adequate; sometimes it became very elaborate, as, for example, that used to measure the oxygen-supplying power of the soil.

Professor and Mrs. Livingston always maintained friendly social relations with students and graduates in their home or at their summer camp site on the Magothy River. Conversations were lively, but they usually came back to plants, which were present in the house and were grown under various conditions, including water culture, in the home gardens. In these general conversations, politics, religion, philosophy, literature, and the arts came up, but they were of secondary interest to plant science; all were approved, however, as human activities having value both in the maintenance of the social order and in the development and recreation of personality.

Though the periods of sustained philosophical discussion were few, both in lectures and in conversations with students severally or in groups, brief allusions to Dr. Livingston's scientific point of view were quite frequent. He reacted against the "teleology or even out-and-out mysticism" that characterized most writers in biological science up to the turn of the century; as he stated, "My *Weltanschauung* seems always to have been of the aetiological rather than of the teleological sort." This point of view evidently was not studied nor consciously adopted; it was a thoroughly characteristic part of his personality, a result of his own reactions to all the influences and personalities he had met during his lifetime.

His life was almost completely *expressed* during his thirty-one years (1909–40) in the Laboratory of Plant Physiology of the Johns Hopkins University; it seems to have been *shaped* almost as completely by his experiences before he accepted the professorship there.

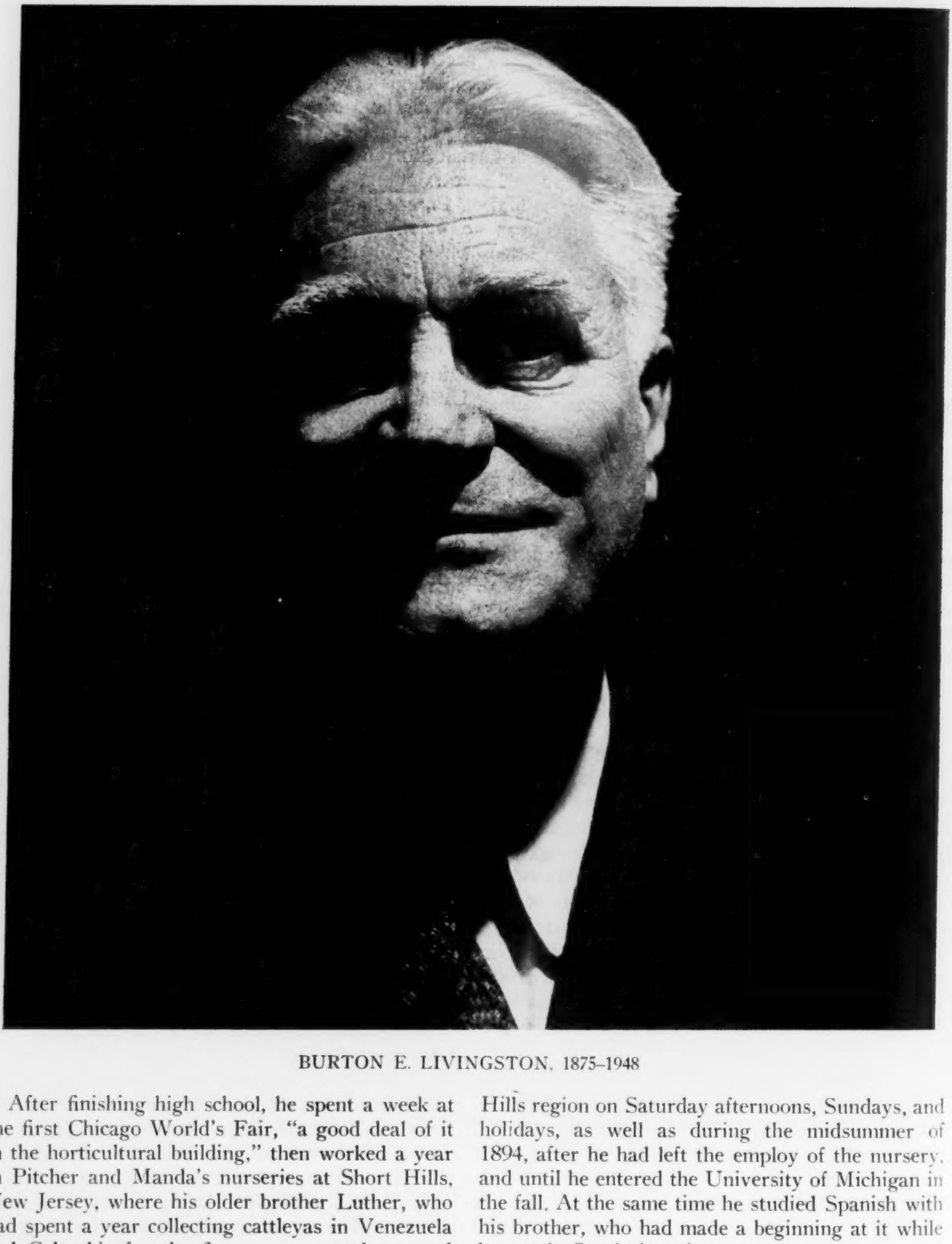
Dr. Livingston's birthplace and boyhood home was on the outskirts of Grand Rapids, Michigan; his parents were farm-reared, but in the rapidly growing city his father was a street grading contractor. The home lot was large enough, however, to express the farm-fostered interests of his parents. There were vegetable and flower gardens, collections of wild flowers, and ornamental plants outdoors and many potted plants indoors, including an orange and a lemon tree grown from seeds that Burton planted when he was less than four years old.

Parents and older brothers and sisters were especially interested in plants; and this interest, which probably came naturally to him, was heightened in Burton by many excursions with older brothers to near-by bogs, streams, and woodlands in which the native vegetation grew undisturbed, as well as to upland meadows and fields with their cultivated crops. They called many plants by their Latin names, and Dr. Livingston could not recall the time when he did not recognize the two *Hepatica* species native to that region as *triloba* and *acutiloba*.

Another feature of his home environment, which exerted a strong influence, was the fact that during the winter months his father's tools and equipment were at home, where he could repair them for use during the following summer; the boys assisted him, acquiring skill in simple mechanical operations and wood-working. With tools at hand, the boys naturally employed them to construct devices for their own use, including, on Burton's part, canoes, sleds, glass-covered trays for a butterfly collection, and presses and cases for plants. Books of a great variety and a small microscope were at hand for leisure reading and study.

His high-school training, begun when he was fifteen, was characteristic of the time and included a considerable range of natural science. He accomplished the equivalent of three years of Latin and one of Greek as extras, studying them by himself during the summer; this home study indicated his avid interest in languages, which was maintained throughout his life, as well as a penchant for acquiring knowledge independently.

Books and magazines were the foremost recreation of the Livingston family. "Everyone in the family circle read and read," according to Dr. Livingston. The children played the card game "Authors," became acquainted with the portraits and titles of noted authors, and read many of their works as they grew older. Burton read "dozens and dozens of boys' books," Longfellow and Bryant, parts of the great religious books of the world, and Thoreau, Emerson, and Carlyle before he finished high school. He bought a morocco-bound set of Shakespeare's works with money earned by mowing neighbors' lawns.



BURTON E. LIVINGSTON, 1875–1948

After finishing high school, he spent a week at the first Chicago World's Fair, "a good deal of it in the horticultural building," then worked a year in Pitcher and Manda's nurseries at Short Hills, New Jersey, where his older brother Luther, who had spent a year collecting cattleyas in Venezuela and Colombia for the firm, was a cataloger and bibliographer. His labors acquainted him with a great variety of cultivated plants, and he extended his acquaintance to the wild plants of the Short Hills region on Saturday afternoons, Sundays, and holidays, as well as during the midsummer of 1894, after he had left the employ of the nursery, and until he entered the University of Michigan in the fall. At the same time he studied Spanish with his brother, who had made a beginning at it while he was in South America.

His herbarium, begun in high school, and added to as a hobby until the fall of 1894, earned him ten hours of advance credit at the University and.

with his high-school course, constituted all his schooling in taxonomic botany. At the University, he worked at a plan to catalog the plants in the region of Ann Arbor according to habitat, with the sympathetic interest of V. M. Spaulding. During his first college summer vacation, he read works on plant geography in the botany building at Columbia University while visiting his brother Luther in New York, and at that time became acquainted with N. L. Britton and John K. Small. On his return to Ann Arbor, he studied plant physiology under F. C. Newcombe, became his assistant in 1895, and continued in that capacity until graduation in 1898.

He taught the natural-science courses in the Freeport, Illinois, high school during the first year after his college graduation, and then became a Fellow and assistant to Professor C. R. Barnes at the University of Chicago, under whom he had charge of the laboratory exercises in plant physiology until 1905. He became deeply interested in ecology, which was being developed at Chicago largely through the work of H. C. Cowles. He continued his work begun while a senior at the University of Michigan on the influence of the osmotic value of solutions on a fresh-water alga, and published *The Role of Osmosis and Diffusion in Plants* in 1903. At Chicago he attended the lectures of J. M. Coulter in plant morphology, Jacques Loeb in general physiology, and F. Lengfeld and J. O. Stieglitz in chemistry. The Ph.D. degree was conferred upon him by the University of Chicago in 1902.

While he was a Fellow at the University of Chicago, he spent one summer recess teaching botany at the Eastern Illinois State Normal School, and another at the New York Botanical Garden, where he became acquainted with its director, D. T. MacDougal, and with F. E. Lloyd, H. M. Richards, and C. C. Curtis, fellow-workers with whom he maintained a lifelong friendship.

The summer of 1905 was spent at the Desert Laboratory of the Carnegie Institution of Washington, at Tucson, Arizona, and the early winter of 1905–06 in the U. S. Bureau of Soils, where he was engaged in soil-fertility investigations. He was a staff member of the Carnegie Institution of Washington from 1906 to 1909, part of which period he spent at the Desert Laboratory, working under MacDougal's direction on problems of physiological ecology, mainly on the relations of plants to soil moisture and to evaporation; he also spent some time at the Missouri Botanical Garden with William Trelease, H. von Schrenk, and J. A. Harris; and the year 1908 in Europe, mainly at Munich with von Goebel, Hegi, and Renner, but also at Leipzig with Pfeffer, and at Heidelberg with Klebs and Gluck; he made, besides, trips through Germany and Switzerland and a brief visit to the Rothamsted Station in England.

On his return to America he attended the AAAS meetings of 1908 in Baltimore, and there saw Professor Johnson and his newly established botanical garden on the Homewood tract, soon to become the campus of the Johns Hopkins University. This meeting led to an offer of an appointment as professor of plant physiology at the Johns Hopkins University, sent to him by telegraph while he was on a tour of southern California with MacDougal and several visitors to the Desert Laboratory, in the early summer of 1909.

Professor Livingston once told me that he found Lon A. Hawkins waiting on the steps of his laboratory on his first day in the new position; Hawkins announced that he would be his first student. Other students during that first year were F. H. Blodgett, W. D. Hoyt, W. R. Jones, L. W. Sharp, H. H. York, and W. H. Brown. With members of this group, Professor Livingston designed and constructed new equipment for the laboratory, and, when the new building which was to serve during the remainder of his professional life was erected in the winter of 1911–12, together they designed the interior details and installed some of the equipment. He once said that he had "gained as much from those students as they gained from [him]."

The summers of the first several years after he joined the faculty of the Johns Hopkins University were spent at the Desert Laboratory, carrying forward studies on soil moisture and evaporation supported by grants from the Carnegie Institution, and with the help of different assistants. He frequently acknowledged his indebtedness to these men: W. H. Brown, J. S. Caldwell, E. M. Harvey, H. E. Pulling, J. W. Shive, A. L. Bakke, H. C. Sampson, E. S. Johnston, J. D. Wilson, G. S. Fraps, and L. J. Pessin.

In 1921, he spent the summer with Mrs. Livingston at the Desert Laboratory, completing studies begun and carried on with Forrest Shreve during ten years. That year he published *The Distribution of Vegetation in the United States, as Related to Climatic Conditions*. Two later summers, those of 1922 and 1926, also were spent at the Desert Laboratory.

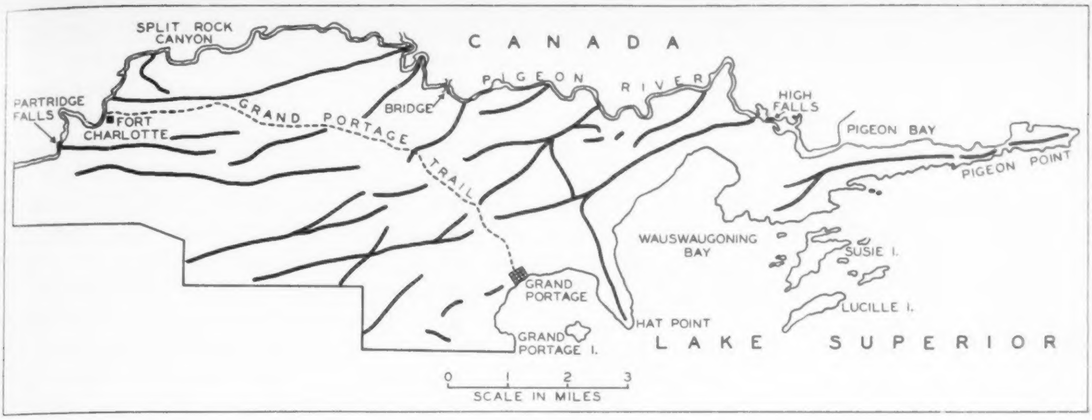
The studies carried on by Professor Livingston and his students in the Laboratory of Plant Physiology at the Johns Hopkins University dealt with the influences of all phases of the environment—temperature, radiation, atmospheric and soil mois-

ture and oxygen composition of the atmosphere, air movement and salt content, other dissolved or colloidal substances, and osmotic value of the soil or nutrient solution—on the major plant processes, transpiration, respiration, absorption, photosynthesis, germination, growth, configuration, vigor, pathology, development, and distribution of plants. Much attention was paid to the dynamic aspects of the environment, the rates at which substances or energy are made available to plants and at which plants absorb, transform, and utilize them. Physical and chemical properties of environmental phases were studied apart from their relations to plants in many instances, and practical applications to agriculture, horticulture, and forestry received considerable attention. A list of publications from the laboratory, from 1909 to his retirement to the status of emeritus professor in 1940, by Professor Livingston and his students and associates, includes 285 titles, six of which were those of books containing 360–979 pages. Those who worked or studied in his laboratory numbered 137, and of these, 28 received the Ph.D. degree from the Johns Hopkins University with him as their major professor.

Professor Livingston's energies were extended to the dissemination of knowledge by means other than teaching, lecturing, editing, and writing. He was active in the affairs of the American Association for the Advancement of Science throughout most of his professional life; he was its permanent secretary (1920–31), general secretary (1931–34), member of the Executive Committee (1920–46), and chairman of the Executive Committee (1941–45). During his service as permanent secretary, he began several activities to increase the value of the Association to its members, among which were the publication of the *Preliminary Announcement*, *Reports* of the Annual Meetings, and periodic *Summarized Proceedings*. He is given credit by his associates for placing the financial affairs of the Association upon a secure basis, and for introducing or developing general sessions and popular lectures, as well as the Exhibitions, press services, Association Prize, secretaries' conference, Academy conference, and *General Program* as features of the Annual Meeting of the Association.

He was active in the American Society of Plant Physiologists from the drafting of its constitution and the organization of its financial program, in which he played a conspicuous part, to the end of his life. He was its president in 1934, was elected its first Barnes Life Member, and was awarded its Stephen Hales Prize in 1946. Other societies of which he was a member and to which he contributed energies and resources were the National Research Council; the American Philosophical Society; the Botanical Society of America; the Ecological Society of America; the American Society of Naturalists, of which he was president in 1933; the American Academy of Arts and Sciences, of which he was a Fellow; and the honor societies of Sigma Xi and Phi Beta Kappa, the latter of which he served as a member of its editorial board.

He died on February 8, 1948, one day before the seventy-third anniversary of his birth. He is survived by his second wife, the former Marguerite A. Brennan Macphilips, to whom he was married in 1921, and with whom he collaborated throughout their married life, in the production, standardization, and distribution throughout the world of porous porcelain atmometers, cones for measuring the moisture-supplying power of the soil, and autoirrigators, as well as in other phases of his work, studies, and travels. His life is a distinguished example of what the traditional American family, home, and educational environment are capable of producing.



A FAMOUS WILDERNESS HIGHWAY

KARL VER STEEG*

Professor Ver Steeg (Ph.D., Columbia, 1930) has been teaching at the College of Wooster since 1923 and is now head of the Department of Geology there. He became interested in the Grand Portage while working on a problem in geology in northeastern Minnesota, and carried out the field work in the summer of 1946 under a grant from the William H. Wilson Research Fund.

NEARLY one hundred and fifty years ago the Grand Portage, one of the oldest and most important highways in North America, was abandoned. Today, deer, bear, and moose wander across the route of this ancient forest highway, which was once used by thousands of Indians, fur traders, and *voyageurs*. It is now overgrown with vegetation, but one can still trace its course, in places, by the depressions worn by the trampling of thousands of feet in the moss and forest duff. Majestic pines, spruce, and white birches line the trail over which passed the traffic of half a continent. In this wilderness the names are centuries old.

The geological and geographical factors responsible for the Grand Portage are interesting examples of the effect of surface features on human activities. A study of the map reveals the Pigeon River flowing in an easterly direction from the site of old Fort Charlotte. This crooked stream flows nearly 20 miles to reach a point only 12 miles distant as the crow flies. Below Partridge Falls, the Pigeon River has deep, steep-walled, rocky gorges; one of these, Split Rock Canyon, is spectacular. The lower course of 20 miles to Lake Superior is interrupted by many falls and rapids; the largest is High Falls (90 feet).

Bordering Pigeon River on both sides is rugged country; on the Canadian side the land is too mountainous and the distances are too great for portaging to be practicable. It is almost impossible to navigate the lower Pigeon River by canoe or boat and equally difficult to find a good portage above it. Grand Portage Bay extends so far back into the southwest-trending shoreline of Lake Superior that the distance to the Pigeon River above the falls and rapids is considerably shortened. Along Pigeon River the distance is 20 miles and difficult to traverse, whereas the more direct route by way of the Grand Portage is less than 9 miles.

The region back of the village of Grand Portage and in the immediate vicinity is also rugged, the ridges being high and steep-sided, often with bluffs. These hills, which stand several hundred feet above the level of Lake Superior and the adjacent valleys, look almost like mountains and form a nearly continuous, impassable barrier in an east-west direction. There is a series of deep, broad gaps through the ridges directly in line with the village. These gaps afford a gradual and rather low gradient from Grand Portage Bay to Pigeon River below Partridge Falls near Fort Charlotte.

* The author is indebted to Drs. G. M. Schwartz and F. F. Grout, Geology Department, University of Minnesota, for the loan of manuscripts. Other source material was The Story of the Grand Portage (Solon J. Buck: *Minn. History Bull.* **5**, 14–27, 1923) and *The Voyageurs' Highway* (Grace Lee Nute: Minn. Historical Soc.).

I found the walk along the trail not difficult as compared with portages having steep ascents over rocky outcrops. With the exception of local undulations—usually of gentle gradient—the trail is a gradual ascent from Grand Portage Bay through the gaps in the ridges. In the first 1.6 miles from Lake Superior, the rise is about 211 feet per mile; the remainder of the distance to Pigeon River has an average gradient of about 40.5 feet per mile. Thus, most of the rise takes place in the first few miles from Grand Portage Bay. Occasionally, muddy and swampy conditions after heavy rains make the portage more difficult.

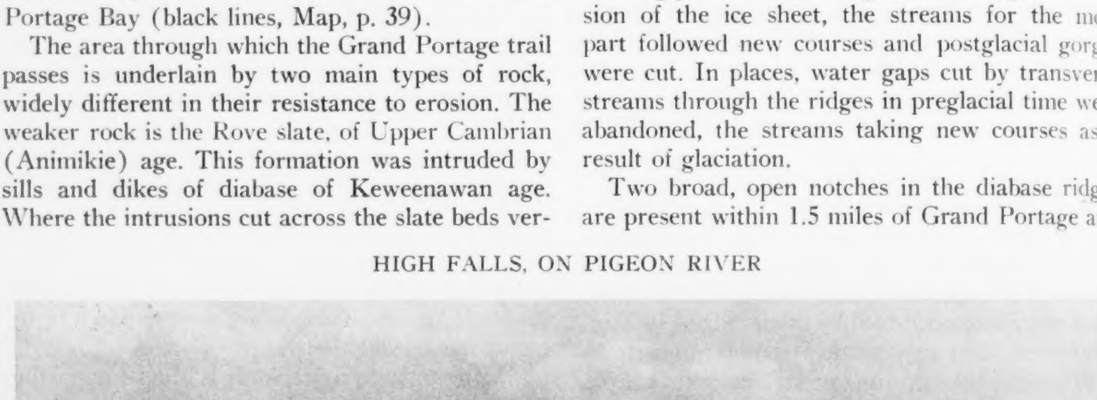
The characteristic topography of the area north of the village of Grand Portage extends well beyond Pigeon River into Canada and westward for 55 miles to Gunflint Lake. It consists of a belt of ridges stretching for the most part in a nearly east-west direction, having a northeast-southwest trend in the area north and northwest of Grand Portage Bay (black lines, Map, p. 39).

The area through which the Grand Portage trail passes is underlain by two main types of rock, widely different in their resistance to erosion. The weaker rock is the Rove slate, of Upper Cambrian (Animikie) age. This formation was intruded by sills and dikes of diabase of Keweenawan age. Where the intrusions cut across the slate beds vertical walls, or dikes, were formed. The formations have been folded, and the slate beds and diabase sheets now tilt at a gentle angle (5°–15°) toward Lake Superior. The topography developed on rocks of unequal hardness, having a dip in one direction (monoclinal) toward the south and striking about N. 70° E., or nearly east and west, is that of a series of parallel unsymmetrical ridges with the steep side facing the north and long gentle back slopes to the south. This topography resembles the Appalachian Ridge and Valley surface features, the only difference being that it is on a smaller scale, and the prevailing dip of the hard rock beds which constitute the ridges is mainly in one direction and averages lower.

The valleys have been modified by glaciation, which scoured the ridges to some extent and deposited debris in the valleys, thus changing the preglacial drainage from streams to chains of lakes running parallel to the ridges. Following the recession of the ice sheet, the streams for the most part followed new courses and postglacial gorges were cut. In places, water gaps cut by transverse streams through the ridges in preglacial time were abandoned, the streams taking new courses as a result of glaciation.

Two broad, open notches in the diabase ridges are present within 1.5 miles of Grand Portage and

HIGH FALLS, ON PIGEON RIVER



another in the ridge immediately to the north of the village, 2.5 miles from Lake Superior. A fairly large stream must have occupied these gaps in preglacial time. The present stream flowing through the gaps, Grand Portage Creek, is a misfit, a small postglacial stream occupying a large valley, the former site of a much larger river.

Beyond the last important gap, about 2.5 miles from Grand Portage Bay, the trail turns to the west and in a broad curve follows an almost level route on the back slope of a gently dipping diabase ridge, avoiding the great swamp that extends almost directly eastward from Fort Charlotte. It appears that Grand Portage trail follows a natural route connecting Lake Superior with the great chain of interior lakes to the northwest and that it was a preglacial river course cut through the gaps in the ridges.

The present inhabitants of the village of Grand Portage number about 300 and are mainly descendants of the Chippewa (Ojibway) Indians, survivors of the once-powerful, warlike tribe that occupied the site from time immemorial. These Indians have been immortalized in the famous poem "Hiawatha." Longfellow went to the wilderness of the north shore of Lake Superior for his hero and to the *Kalevala*, an old Finnish epic, for the form of his poem.

Not far from Grand Portage, at a spot on Hat Point overlooking Wauswaugoning Bay, stands the celebrated Witches' Tree, on a rock pedestal rising from Lake Superior (*Gitchi Gummi*). Indian legend relates that this old gnarled cedar is inhabited by an evil spirit having the form of a dark-brown, eaglelike bird. The Indians avoided it as a symbol of bad luck. Today, this noble old tree, which probably antedates the earliest explorations in the region (it is believed to be four hundred years old), stands with its roots firmly embedded in cracks in the rock, like a sentinel breasting the strong winds of Lake Superior.

MIKE FLATT

A HEREDITARY CHIEF OF THE CHIPPEWAS. HIS LONELY CABIN IS JUST ABOVE THE PRESENT VILLAGE OF GRAND PORTAGE.

CHIPPEWA MEDICINE MAN

ALEX POSEY, SOMETIMES CALLED MA-MUS-QUASH.

History tells us that the fierce Sioux Indians first occupied the forests of Minnesota and that they were driven out by the Chippewas during long generations of warfare. The Sioux then became Plains Indians, and the Chippewas continued to be forest dwellers. The Grand Portage and the border lakes area to the west were the sites of many battles between the Sioux and the Chippewas.

One of the remarkable landmarks of this region is Picture Rock, overlooking Crooked Lake on the Voyageurs' Highway. This rock was described by Alexander Mackenzie, the celebrated explorer and trader of the North West Company, who passed along the Grand Portage route about one hundred and sixty years ago. Its smooth face was split and cracked in different parts, and into one of its horizontal chasms, he remarks, "A great number of arrows have been shot, which is said to have been done by a war-party of Sioux who had done mischief in the country and left their weapons as a warning to the Chebois [Chippewa]."

RECONSTRUCTED STOCKADE AND HEADQUARTERS OF THE NORTH WEST COMPANY

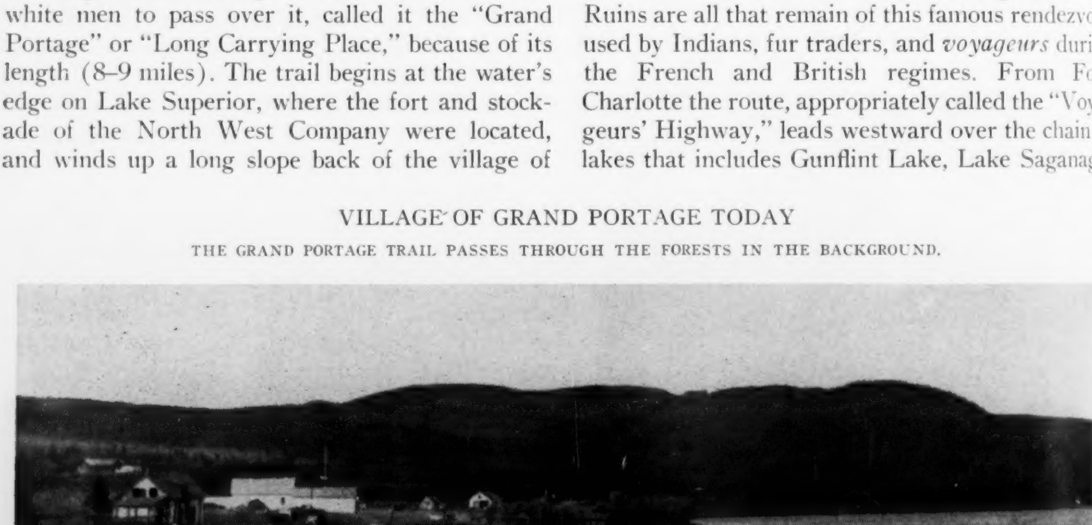
GRAND PORTAGE ISLAND AND HAT POINT IN THE DISTANCE.

The Grand Portage was used by the Indians for generations before the advent of the white man. It was the shortest distance between Lake Superior and the Great Lakes region and the far Northwest. The Indians called the trail *Kitchi Onigum,* meaning "Great Portage." The French, the first white men to pass over it, called it the "Grand Portage" or "Long Carrying Place," because of its length (8–9 miles). The trail begins at the water's edge on Lake Superior, where the fort and stockade of the North West Company were located, and winds up a long slope back of the village of Grand Portage to the cabin of Mike Flatt, the Chief of the Chippewas. This interesting old Indian possesses medals, uniforms, and a British flag presented to his ancestors.

From Mike's lonely cabin the trail leads through the forest to Fort Charlotte on Pigeon River. Ruins are all that remain of this famous rendezvous used by Indians, fur traders, and *voyageurs* during the French and British regimes. From Fort Charlotte the route, appropriately called the "Voyageurs' Highway," leads westward over the chain of lakes that includes Gunflint Lake, Lake Saganaga,

VILLAGE OF GRAND PORTAGE TODAY

THE GRAND PORTAGE TRAIL PASSES THROUGH THE FORESTS IN THE BACKGROUND.



Lac-la-Croix, Rainy Lake, and Lake of the Woods to Lake Winnipeg, from which the *voyageurs* scattered far and wide to their respective trading posts—to Hudson Bay, Assiniboine, Saskatchewan, Peace River, Lake Athabaska, and even to Great Slave Lake, Oregon, and the Pacific Coast.

Grand Portage was the first town and trading post in Minnesota, its activities preceding the earliest white settlement in that state by two generations. As early as 1767 it was an important rendezvous and trading post, and at the time the Declaration of Independence was signed it was a busy emporium, the center of the Northwest fur trade.

Over the Grand Portage trail passed many celebrated characters of the early period of exploration. First came the Chippewas, Cree, and Sioux, then the French, English, Scotch, Yankees, and *voyageurs*. From the far-flung reaches of the great Northwest wilderness, as far away as the Pacific Ocean, Mackenzie River, Hudson Bay, and the Canadian Rockies, came the furs to the great North West Company. The Northwesters were, in fact, the men who explored the northern two thirds of the continent. They traced routes to the Arctic and the Pacific. They discovered the passes through the Canadian Rockies. Storms, hostile Indians, conspiracy, violence, murder, and illness took their toll of these adventurous explorers and fur traders.

Radisson and Groseillers, in 1665, were probably the first white men to know of the Grand Portage. Other great explorers who passed over it were Sieur de la Verendrye (1791), the French explorer, and Alexander Mackenzie (1789), the fur trader of the North West Company who discovered the Mackenzie River and reached the Pacific Ocean in his search for a northwest passage. He was the first white man to cross the continent in northern latitudes. David Thompson, astronomer, explorer, and trader, said by competent authorities to have been the greatest practical land geographer the world has ever known, arrived at Grand Portage in 1797. The North West Company employed him to follow the 49th parallel and determine the location of the company's trading posts. This intelligent man mapped a large area in the northwest wilderness under difficult conditions. In 1822 he returned to Grand Portage to serve as the British member of the International Boundary Commission.

Dr. John Bigsby, a member of the David Thompson party, served as an artist and physician. He gave us our first authentic pictures of the border lakes, and his sketches of the Indians along the canoe route are a valuable contribution to historical records. He was secretary of the International Boundary Commission.

McFARLAND LAKE

SHOWING THE LOW RIDGES WITH STEEP SCARPS TO THE NORTH AND GENTLE BACK SLOPES TOWARD LAKE SUPERIOR.

The celebrated North West Fur Company was organized in Montreal in 1784. It dominated the shores of Lake Superior with relentless severity, expelling all other traders; its men were "Lords of the Lake." The story of the North West Company is one marked by despotism and immorality, as well as by adventure and courage. The traders debauched the natives and became immensely wealthy from the profits derived from the rich cargoes of furs. They monopolized the fabulous fur trade of half a continent.

The X Y Company was organized by wealthy men who had been excluded from the North West Company. The jealousy, violence, and murder that followed were finally ended when the two companies merged. The North West Company also warred with the Hudson's Bay Company, and the same violence and bloodshed marked their commercial competition. They destroyed one another's forts and trading posts and shot one another's agents. This bitter feud was carried to the courts and to Parliament and was finally ended by a compromise and consolidation in 1821.

The end of the period of French domination in the Grand Portage and border lake region came with the loss of Canada in 1760 to the British. Up to the time of the French and Indian wars,

traders pushed to great distances into the apparently boundless Northwest. In 1761 and 1762 British traders arrived at Grand Portage, only to find the Indians very hostile, since they did not welcome the British as substitutes for the French. The next year Pontiac's Conspiracy and the massacre of the British garrison at Mackinac caused the cessation of British trade in the Northwest until 1765. From then on, however, the English dominated the Grand Portage and the boundary lakes region until 1796, when the post at Mackinac was occupied by American troops. It was rumored that at Grand Portage revenue agents would collect duty on British goods entering the United States there. The North West Company found another route to the interior by way of Kaministiquia River and erected new headquarters at Fort William at its mouth. The X Y Company remained at Grand Portage until it merged in 1804 with the North West Company. The Grand Portage trail was then abandoned.

THE fur trade introduced the white man's luxuries to the Indians. Brandy, rum, and wine constituted the wet goods; guns and ammunition came next in importance, then dry goods, hardware, and trinkets. Beaver skins were the most important of the furs; in fact, the fur trade was sometimes called the "beaver trade." The barons who controlled the North West Company lived in splendor at Montreal and Sault Ste Marie. At Grand Portage, their western headquarters, the furs were gathered, baled, and shipped to Montreal, at that time the great fur market of the continent. The workers in the fur trade at Grand Portage were nearly all Canadian, French, or French half-breeds. Those of French extraction were known as *coureurs de bois* ("rangers of the woods") or *voyageurs* ("travelers" or "canoe men"). They worked for a body of Scotch superintendents and owners, businessmen who organized the fur trade, sorted furs, and purchased goods. These courageous and industrious men constituted the North West Company.

Throughout the months of July and August, Grand Portage was a scene of great activity. Goods for trade came in fleets of canoes from Montreal, accompanied by officials of the North West Company, who met other traders coming from the northwest wilderness. The annual meeting was held, and arrangements and agreements made for the following winter. Over hundreds of lakes and portages, through wild rapids and cataracts, came the birchbark canoes loaded with furs. In the canoe yards of the North West Company as many as 75 of these canoes were constructed annually for the fur trade, since they were the only practical mode of transportation for that purpose.

As many as 3,000 men—*voyageurs,* Indians, officials, and traders—congregated in and around the fort and stockade at Grand Portage. Furs and supplies were interchanged, pelts were sorted and baled, and accounts were settled. Then there was a grand frolic: gallons of rum were issued, and the strains of the violin and the skirl of the bagpipe were heard from morning till night. In the great banquet hall, 60 feet long, tables groaned with the weight of game and fish. The dancing lasted till morning. At least 1,500 people of both sexes assembled at the celebrations, and 100 large and 200 small canoes were moored in the navy yard. At the banquets, financial tycoons from Montreal ate and drank side by side with the half-breeds, *voyageurs,* and the untamed Crees and Chippewas from the far Northwest. Such were the scenes at Grand Portage at the time of the signing of the Declaration of Independence.

Paulie Swan

ARTIST'S SKETCH OF A *VOYAGEUR*

HUNGRY JACK LAKE

ON THE VOYAGEURS' HIGHWAY TO THE GREAT NORTHWEST.

During July and August the North West Company employed as many as 500 men at Grand Portage. Supplies and furs had to be transported between Fort Charlotte and the eastern end of the trail. The round trip of almost 17 miles was accomplished in about six hours. The size of the burdens

LOON LAKE

ALSO ON THE VOYAGEURS' HIGHWAY.

carried across the portage by the *voyageurs* is almost unbelievable. Two, and sometimes three, bales of furs were carried on each trip, and an equal weight in supplies on the return journey. During wet weather there were places where the men waded knee-deep in the slippery mud. In September, the traders shouldered their canoes and, by way of the Grand Portage and the great chain of interior lakes to the west, scattered throughout the Northwest for another winter.

The most interesting characters of the fur-trading days were the *coureurs de bois*. These eighteenth-century bush rangers forsook their homes and families and took to the woods to engage in the beaver trade. The movement became so general that whole communities were virtually deprived of their male population, and much destitution resulted. Adventurous youths, and even soldiers of the regular garrison, took to the forest and lived the free life of the Indians, trading with them and often marrying among them. Always loyal to the French, their alliances with the Indians gave them great ascendancy over the savages and kept the natives friendly to the French.

The *voyageurs* were the pioneers of commerce on Lake Superior. Mostly French or *Brûlés* ("half-breeds"), these swarthy men were truly the heroes of the canoe. A better canoe man than the Indian, the *voyageur* handled a canoe in the rapids and fast water with phenomenal skill. His ability to live in the wilderness, make canoes, erect forts, manage sled dogs, and procure furs made him the mainstay of the fur trade.

For generations, the canoe songs, which were folk songs inherited by the French Canadians from the French of the Loire Valley, were heard along the shores of Lake Superior. It was a great sight in those days to see a fleet of canoes with the oars moving in cadence with one of the rollicking folk songs. The *voyageurs* were polite, jocular fellows who worked hard and took orders well, but who nevertheless assumed little responsibility and, on the whole, took life easily. Fickle, reckless, and immoral though they were, they were liked by the Indians, especially the Indian women.

Pemmican was the main item of food for the fur trader. This concentrated food was made of dried buffalo meat, into which were pounded dried berries and marrow fat; the whole was packed in a rawhide bag. It was put up without salt so it could be eaten without producing thirst. Pemmican, made at prairie forts in the buffalo country of western Canada and shipped by canoe, made Grand Portage and the fur trade possible.

In his mode of dress the *voyageur* was picturesque; he loved vivid colors. His cap was of bright-red woolen material; his shirt was short, made either of buckskin or woolen cloth. He wore a pair of deerskin leggings, which reached from his ankles to a little above the knee and were held up by thongs attached to a belt about his waist. He wore a breechcloth like that of the Indians, and on his feet he wore moccasins without stockings. His thighs were bare. His coat, with hood attached, was of some gaudy color—often blue. To complete the costume, he tied a brightly colored sash about his waist. A beaded or otherwise ornately decorated bag hung from the belt in which he carried his pipe and tobacco and other objects.

These men performed the almost incredible feat of crossing and recrossing the continent in birch-bark canoes in a single season. They would start in a canoe from the Columbia River on the Pacific Coast in April and, by threading their way through rivers and lakes over hundreds of portages, shooting the rapids, portaging over mountains without halt in fair weather or foul, sleeping only four hours in twenty-four, they would reach Grand Portage on Lake Superior by the first of July with the regularity of a steamboat. Returning across the continent with equal precision, they would arrive at Fort George at the mouth of the Columbia by the twentieth of October.

Today, only the lapping of the waves on the shores of the beautiful lakes and the whisper of the pines and birches break the silence of the wilderness where once rang the shouts and gay songs of the fur traders. All that remains to remind us of the exploits of these sturdy men of the Northwest are the traces of their trails and portages and the ruins of their forts and stockades along the Voyageurs' Highway.

SCIENTIFIC AND TECHNICAL ENLISTED MEN IN THE ARMY

MARSH W. WHITE

Professor White (Ph.D., Penn State, 1926) has been teaching at his alma mater since 1927 and has been professor of physics there since 1942. During 1944–45 he served as Expert Consultant in the War Department and, in 1945–46, to the Secretary of War. He is now Consultant on Scientific Manpower, General Staff, U. S. Army. Author of several books, his latest is College Technical Physics, *in which he collaborated with R. L. Weber and K. V. Manning (McGraw-Hill, 1947).*

THIS article is a brief report of the activities of the Technical Detachment, an organization established during the latter part of the recent war by the War Department for the preferential selection, assignment, and utilization of enlisted men who were professionally qualified for scientific and technical duties. Data are presented showing the ages, marital status, fields of specialization, academic degrees, work prior to induction, and the branches of the service to which assignment was made.

During the spring and summer of 1944 the needs of the armed services for additional personnel resulted in the induction of an increasingly large number of professionally qualified scientific and technical personnel, many of whom had previously been given a deferred status by Selective Service. This situation became a matter of great concern to both civilian and military groups charged with the responsibility for research and development of military devices and techniques. After consideration of a number of possible solutions to the problems presented by this situation, the War Department on August 30, 1944, directed the establishment of a Technical Detachment. Responsibility for the procedures for the organization and administration of the Detachment was placed in the New Developments Division, War Department Special Staff, Brigadier General William S. Borden, Director. On May 1, 1946, the Research and Development Division, War Department General Staff, with Major General H. S. Aurand as Director, succeeded the New Developments Division. At present, this unit is designated the Research and Development Group, Logistics Division, General Staff, U. S. Army.

The major objective of the Technical Detachment was to ensure the directed assignment of professionally qualified scientific and technical personnel upon their induction into the Army or to reassign men already in the service when it was learned that they were not being utilized in their technical capacities as fully as they might be. By locating places in the research and development installations having a need for technically qualified enlisted men, the Technical Detachment acted to conserve the skills of such personnel and made possible their exploitation in the development, testing, and application of the newest devices under development by the Army. The Technical Detachment was admittedly an experimental approach to the problems involved in the individually directed assignments of scientists and technical personnel with rare skills inducted into the Army. It was hoped through the functioning of the Technical Detachment to ensure their effective utilization in the Zone of Interior laboratory installations and on teams for the introduction of newly developed devices into the theaters of operations.

In the first few months following the establishment of the Technical Detachment, assignments were limited to men who, as civilians, had participated in the development of special devices for military use. Later this limitation was eased to provide for the inclusion of those who, by education and experience, were peculiarly qualified for such work, even though they had never had actual research experience in these fields. Standards of minimum qualifications for assignment to the Technical Detachment were established. These varied considerably, depending upon the needs of the services and the relative scarcity of various professional groups. For example, physicists were among the scarcest and the most desired, whereas certain kinds of chemists were relatively numerous and at the same time not in great demand. Some types of engineers qualified for research and development, particularly in electronics, were especially needed. The Technical Detachment did not concern itself with the directed assignment of conventional production engineers or nonprofessional technicians.

During the early period of the Technical Detachment, candidates for consideration were ob-

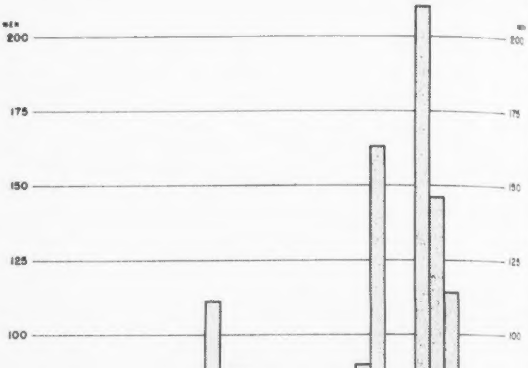
tained from information received from the National Roster of Scientific and Specialized Personnel, the Office of Scientific Research and Development, the Office of Scientific Personnel of the National Research Council, and from individuals, research organizations, industries, and collegiate institutions. Arrangements later were made whereby Army-wide screening of all inductees was conducted at reception centers in order to identify men of the desired technical qualifications. Enlisted men thus located were earmarked as Technical Detachment potentials while their qualifications were carefully examined during the period of their basic military training. For each such man the data obtained from Army personnel records, the National Roster files, and other sources were made into a complete personnel file. Then, in view of the current needs of Army research and development installations, a directed assignment was ordered for all men found to be properly qualified. These assignments were reviewed and approved by a high-level Committee on the Assignment of Scientific and Technical Personnel in the Army. This Committee included representatives from the interested staff divisions of the War Department and from the personnel sections of the Army Service Forces, Army Air Forces, and Army Ground Forces. The Committee had complete authority to assign and transfer any qualified enlisted man in the Army.

It was recognized that the problem of proper utilization after assignment was a difficult and continuing one. To minimize improper placement originally, no man was assigned unless a specific request for his type of skill had been received. To assist in ensuring that the men would not be diverted to nontechnical duties after their original assignments, the orders transferring all men assigned by the Technical Detachment contained the following directive:

> **Subject enlisted man has been selected because of his education and qualifications for scientific and technical work in the Army. He has been classified as a critically needed specialist and will be assigned only to duties which will make full utilization of these qualifications.**

In spite of efforts to ensure satisfactory utilization of men assigned through the Technical Detachment procedures, it is probable that after the completion of their initial tasks many of the duties given to some of the men were not such as to require highly technical competence. Some spot checks were made to determine the degree of effectiveness with which the scientific skills of these men were being exploited. The men themselves frequently notified the New Developments Division concerning their work, particularly when their duties did not seem to be of a sufficiently technical nature. Many of the Technical Detachment men were assigned to an installation on a 60- to 90-day temporary-duty basis. At the end of these periods, the commanding officers had to justify their requests for an extension of the assignments, thus securing a review of duties to which the men had been assigned. In general, it is believed that most of the men were satisfactorily utilized in their technical specialties, but some undoubtedly were eventually diverted from research and development work to less scientific duties.

TECHNICAL DETACHMENT ASSIGNMENTS
SEPTEMBER 1944 THROUGH MARCH 1947.



The Technical Detachment began operations in September 1944 and was discontinued in March 1946, when the induction of scientists under Selective Service ceased. During this period, the qualifications of some 2,500 men were critically examined and directed assignments were made for about 1,800 of this group. Those assigned by the Technical Detachment procedures included 1,056 single men (63 percent) and 614 married men (37 percent). In age, the men ranged from twenty to thirty. Of 1,790 men, 19 percent were between twenty and twenty-three; 17 percent were twenty-four; 17 percent, twenty-five; and 47 percent were between twenty-six and thirty.

The professional occupations of the Technical

Detachment assignees prior to induction are shown in Table 1. In civilian life, 2 percent had been engaged in teaching; 9 percent in research of various kinds; 59 percent in industry (in both production and research); and 7 percent had been students.

TABLE 1

Field of Specialization	No. of Men	Percentage
Chemical engineering	326	18
Biological chemistry	41	2
Organic chemistry	203	11
Physical chemistry	33	2
Inorganic chemistry	92	5
Electrical engineering	171	10
Aeronautical engineering	166	9
Civil engineering	31	2
Mechanical engineering	516	29
Metallurgy	28	2
Physics	89	5
Mathematics	35	2
Zoological sciences	17	1
Miscellaneous	36	2
Total	1,784	100

Out of 1,607 men, 3 percent held the Ph.D. degree; 8 percent, the M.S.; 10 percent, the B.A.; and 79 percent, the B.S. Table 2 shows the numbers of men assigned by the Technical Detachment to the various arms and services in the Army.

The lessons learned from the operation of the Technical Detachment during World War II should provide a partial basis for planning for the mobilization of scientists in the event of another national emergency. The Research and Development Group of the General Staff, United States Army, is cooperating with the Navy, Air Force, and a number of civilian organizations in preliminary work which looks forward to the development of an over-all plan for the effective allocation and utilization of all scientists before and during periods of war. Such a comprehensive plan contemplates the establishment of legislative authority and administrative mechanisms that will be essential in the implementation of the plan. The plan will probably provide that the bulk of scientists qualified for research and development activities shall remain in civilian status for work in OSRD-type organizations and in the laboratories of universities, industries, and the armed services. For the relatively small group that will be inducted into the Army an organization molded along the lines of the Technical Detachment may be included in the plans for mobilization on M-Day and later.

TABLE 2

Assignment	No. of Men	Percentage
Signal Corps	106	6
Ordnance	185	10
Chemical Corps	280	16
Engineers	130	7
Quartermaster	101	6
Medical	90	5
Army Air Forces	728	40
Army Ground Forces	100	6
Others (Manhattan District, etc.)	73	4
Total	1,793	100

IMPOTENCE PRINCIPLES IN MODERN PHYSICS

R. B. LINDSAY

Professor Lindsay (Ph.D., M.I.T., 1924) was an American-Scandinavian Fellow in Denmark in 1922–23. He was on the Yale faculty from 1923 to 1930, when he became associate professor of theoretical physics at Brown University. For the past fourteen years he has been chairman of the Department of Physics at Brown.

THE interest of the distinguished British applied mathematician Sir Edmund Whittaker in problems of physical methodology is too well known to require much comment. His essays in *Nature* and in the *Proceedings of the Royal Society of Edinburgh* are always well thought out, historically well documented, and very stimulating. Would that more mathematically minded people were so enlightening!

In a presidential address to the Royal Society of Edinburgh in October 1941, Whittaker discussed "Some Disputed Questions in the Philosophy of the Physical Sciences" and in the course of his address paid particular attention to those postulates or principles in physics which state categorically our inability to carry out certain processes that are not intuitively impossible. For such statements he introduced the name "postulates of impotence." They might just as well be called "postulates of renunciation." We are all familiar with examples.

Thus, there is the second law of thermodynamics, which tells us in one of its several modes of formulation that it is impossible for any self-acting machine continuously to convey heat from one body to another of higher temperature. Or, again, it is impossible for energy transfer between material systems to take place at a velocity greater than that of light in free space, a postulate closely connected with the theory of relativity. In quantum mechanics we learn from the so-called principle of indeterminacy that it is impossible to make simultaneous measurements of the position and momentum of a particle with the same degree of precision. And so we might go on with the list, even including one or two ancient renunciations, such as that it is impossible to raise water more than 34 feet by means of a lift pump, or that it is impossible to raise oneself by one's own bootstraps!

Such expressions of human impotence both in the realm of the practical and that of the abstract have a peculiar fascination, and the theoretical structures in which they have a place have always seemed to possess a mystery not shared by others in which the postulates have a more affirmative ring. Inevitably, a renunciatory statement in a science like physics, a statement that "you cannot do thus and so," provides a challenge which naturally provokes effort to show that after all it *can* be done. We all know the history of this in the case of the second law of thermodynamics and how many persons have tried to disprove the principle by direct experiment, i.e., by the construction of successful perpetual-motion machines of the second kind. From a practical standpoint, it might be felt that this has been a sheer waste of time and that this has been an unfortunate effect of the statement of the second law in the form of an impotence principle. On second thought, we might, however, conclude that all this essentially futile experimentation has really been a good thing, since there is always the chance that some ingenious mechanical device will come out of it. This has certainly happened in the case of perpetual motion of the first kind in the production of so-called self-winding clocks, etc. The trouble is that, swayed by their conviction of the validity of the impotence postulate, most professional scientists are disinclined to look into the mechanical aspects of so-called perpetual-motion devices, and dismiss them out of hand as idle violations of the postulate.

More important for our present consideration is the *theoretical* standing of renunciatory principles. Whittaker is sufficiently impressed with the theoretical importance of impotence postulates to visualize a future day when any field of physics can be developed along the same methodological lines as Euclid's geometry with impotence postulates as the *a priori* principles and with all the relevant physical laws obtained therefrom by syllogistic reasoning. I am not a prophet and Whittaker may be right, but I should like to examine somewhat critically his point of view from the standpoint of the history and methodology of physics.

We may as well begin with thermodynamics. It must at the outset be admitted that scientists have not been satisfied with the statement of the principles of thermodynamics in impotence form. This is well illustrated indeed by the history of the first law. This, of course, can be expressed in

renunciatory form as the impossibility of producing perpetual motion of the first kind, i.e., the impossibility of constructing a machine that will do work continuously without receiving energy from an external source. Actually, it has been found preferable to state this in *positive* form, namely, that in all the interactions of physical systems the total energy in the universe remains constant. When we reflect that the definition of energy in theoretical physics is based on the existence of a particular invariant of the motion of a dynamical system resulting from a first integration of the equations of motion, and that strictly the energy does not exist when the integration cannot be carried out—i.e., when there is no potential function—we are faced here with a rather paradoxical situation. In no actual terrestrial motion does it prove possible in all strictness to find a potential function and assign an energy; hence, one might suppose that this fact would serve as an excellent principle of impotence. But no such attitude has been adopted; rather, the idea of energy has seemed of such utility and importance that its definition has been extended so as to make its conservation one of the keystones of the theory of thermodynamics. In other words, the difficulty just mentioned has not led us to renounce the use of the energy principle; it has only encouraged us to expand it meaning and extend its significance.

The history of the second law also indicates a more recent desire to get away from the renunciatory element contained in its early formulation. On the purely formal level, the introduction of the entropy concept by Clausius was an attempt to give the principle an affirmative ring and enable quantitative conclusions to be drawn from it. Even the elementary student must have observed that the only way in which one can draw positive conclusions from an impotence statement like the second law in its historical form is by means of the principle of contradiction. This has long been a favorite method of proof of theorems in pure mathematics, but it has never found such favor among physicists. Even some mathematicians now regard it with a jaundiced eye. We may not be impressed with the possible utility of multiple-valued logics in the development of physical theories, but I believe most of us would prefer to avoid the chains of the excluded middle class as far as possible.

As a matter of history, the introduction of entropy in an attempt to replace the impotence implication of the second law by what might be called a *competence* implication has not been deemed sufficient. No physicist nowadays feels satisfied with the principle of increasing entropy except in terms of the statistical point of view, with its emphasis on the probable character of calculated results. On this view, we no longer renounce forever the possibility of finding the uncompensated increase in free energy of an isolated system, but merely look upon this occurrence as extremely unlikely in systems of any degree of complexity. Why do we find this point of view more satisfying? There are doubtless many reasons, but one of them certainly is the removal of the drastic prohibition even to think of the possibility of a violation of the second law in our universe. It makes it possible for us to account for the obvious violations presented by the Brownian motion and many other fluctuation phenomena without subterfuges based on the contrast of microscopic and macroscopic. At the same time it provides a healthy deterrent to any philosopher or theologian who might be inclined to erect a far-reaching metaphysical demonstration (e.g., the existence of God) on the basis of the definitive renunciatory character of the second law. This is, of course, wholly aside from the value of the statistical method in providing a fundamental basis for the whole of thermodynamics, in which thermodynamic principles become deductions rather than independent postulates. Moreover, this also leaves out of consideration the power of the statistical method to answer questions about the specific behavior of matter (e.g., equations of state and problems of energy transport), which thermodynamics is quite powerless to tackle.

THE view that I am propounding here may then be set forth briefly as follows: principles of impotence have a certain value in one stage of the development of physical thought; they fascinate and they provoke thought as to their fundamental meaning. But we should never be contented with them as the final expression of the content of any physical theory. Too firm an adherence to them and too close a preoccupation with them may actually retard the progress of physics.

In any consideration of the reasonableness of this point of view, we are inevitably faced with the necessity of examining what is probably the most famous of all principles of renunciation, namely, the indeterminacy principle of Heisenberg and the associated but more general complementarity principle of Bohr. Both have achieved prominence in recent years, not merely among physicists but among philosophers and the educated laity.

The history of the indeterminacy principle in quantum theory is an interesting one. It will be recalled that, when in 1925 Heisenberg founded

his brand of quantum mechanics, the prevailing mood in the Copenhagen school of Bohr was one of pessimism about the possibility of any further success in applying the Bohr quantum postulates to mechanical atomic models. Though the broad outlines of the theory of polyelectronic atoms seemed well founded, the inability to attain precision in calculating the ionization potential of even such a simple atom as helium was disheartening. And, of course, there were other serious difficulties connected with optical dispersion and collision phenomena. It was then that Heisenberg decided to forego the further use of mechanical or space-time picture models in which electrons, conceived as actual charged particles, were assumed to move in definite orbits. His plan was to build an abstract mathematical formalism in which the only physical quantities entering are directly observable. Relevant examples are the frequencies and intensities of spectral lines. The resulting so-called *matrix mechanics* was remarkably successful in its predictions, even though many physicists called it formal, abstract, and unpicturesque and therefore not understandable by experimentalists accustomed to thinking in terms of pictures. To what extent Heisenberg was affected by these complaints I do not know, but it is a fact that in a famous paper in the *Zs. f. Physik* in 1927 he proceeded to inject some *Anschaulichkeit* into quantum mechanics: the indeterminacy principle was the result.

Why have we reviewed this well-known historical background? Simply to emphasize that in the straightforward use of quantum mechanics there is no need for the indeterminacy principle. Its status as a principle of renunciation has thus a different basis from that of the second law of thermodynamics. It was not discovered as a fundamental restriction on physical theorizing in the atomic domain from which the theorems of quantum mechanics flow as do those of thermodynamics from the second law. It is not only unnecessary for the application of quantum mechanics to the problems for which the theory was designed, but may actually be said to have arisen out of a violation of the very principles on which the quantum mechanics was based, namely, the restriction to observable quantities. This is not to say that the principle has been useless, for it has certainly served to attract attention to problems in the methodology of quantum mechanics. At the same time it has misled many.

Thus the renunciatory element in the indeterminacy principle has been much exaggerated in popular accounts. Possibly this is due to the fact that its usual presentation stresses the disturbance produced in an atomic system by the attempt to measure any quantity associated with it. This in turn is closely related to the disturbing interaction of any physical measuring device with the system under measurement. But this is misleading. Classical physics has never questioned the necessity of accounting for the disturbance produced by a measuring instrument on the system being measured. Thus, the introduction of a voltmeter across a portion of a circuit changes the circuit, and its reading must be "corrected" if the drop in potential existing before the insertion of the meter is to be obtained. From another point of view, the operation of inserting the meter, reading it, and making the corrections, taken together, constitute what we mean by the concept *potential difference*, though we later generalize this by the introduction of more general abstract concepts. The same situation is met throughout physics. It is our hope that the assignment of numbers to these concepts can be carried out consistently enough to enable us to test unambiguously in the laboratory the deductions of theory. The well-known fact that laboratory measurements are always given in terms of sets of numbers to which some sort of statistical analysis must be applied in order to obtain a unique answer could lead logically to an impotence postulate stating that precise physical measurements are impossible. It has not been thought worth while to introduce such an utter renunciation.

Actually, the indeterminacy principle considered from the standpoint of disturbance has been applied largely to mental experiments and measurements and not to those performed in the laboratory. This is not to deny that it has not been useful in some cases. Thus it can associate the mean breadth of spectral lines with the mean lifetime of an atomic stationary state. But it is well to point out that this result has nothing essentially to do with the renunciation of precision of measurement due to the disturbance produced by the measuring instrument. The indeterminacy principle in its useful form is simply a relation between the standard deviations of quantum mechanical canonically conjugate quantities as derived in straightforward fashion from the postulates of quantum mechanics. In this there is certainly no more expression of impotence than there is in the well-known statement that in a finite wave train the shorter the train the greater is the spread in frequency required for its adequate representation by a Fourier integral. As a matter of fact, it was this type of representation which DeBroglie used in his foundation of wave mechanics. It is a definitely affirma-

tive approach with no element of renunciation involved in it—unless we wish to take the stand that all physical theorizing is renunciatory.

And so it seems to me that, though the indeterminacy principle (and here I include also the wider generalization, the complementarity principle of Bohr) has a certain theoretical fascination, its contribution to the future of physics as a principle of impotence is not sufficiently profitable to justify the attention that has been paid to it. It is naturally the kind of physical statement that appeals to many philosophers who in general are not in a position to study minutely its precise meaning in physics and hence are tempted to erect on it philosophical generalizations bearing little justifiable relation to its physical meaning. Many such philosophical misinterpretations have been pointed out and discussed by Philipp Frank and need not be further emphasized here. We may merely mention the numerous attempts to use the principle to establish the freedom of the will as a philosophical principle. The attempted application of complementarity to biological problems and the nature of life must be regarded, in spite of Bohr's ingenuity, as of highly questionable scientific value.

It is only natural that Whittaker should have numbered among impotence postulates the Einstein principle of special relativity in the form that "it is impossible to detect uniform translatory motion with respect to the primary inertial system by any means whatever." Unlike the indeterminacy principle, the principle of special relativity in its impotence form would appear to have an unimpeachable historical basis in the famous Michelson-Morley experiment, with its attempt to measure motion of the earth relative to the luminiferous ether and its so-called null result. Most popular presentations of the theory stress this aspect. But when we examine the situation more closely, the issue is not so clear-cut.

In the first place, the experiment even as originally performed never gave a completely null result, and I presume most physicists are familiar with the long-continued, often-repeated, and admittedly careful experiments of the late D. C. Miller. These also led to a positive result, though by no means as great as that expected on the original Maxwell prediction. No one questions the historical importance of the failure of the experiment to give the full predicted result in suggesting to Einstein the postulate of special relativity. But it must also be recognized that the great contributions of the special theory to physics in the mass-energy relation, etc., have long since rendered it impregnable against attack based on an eventual positive result of experiments like those of Michelson, Morley, and Miller. By the same token the impotence or renunciatory aspect of relativity has become of relatively minor significance. The really important contributions of relativity have come as positive deductions from the Lorentz-Einstein transformation equations, and the test of the theory is in the experimental verification of these deductions. And it seems to me that it is ultimately less misleading to phrase the fundamental postulate of special relativity in the positive form—"the laws of physics have the same form in all inertial systems"—rather than in the impotence form to which this indeed logically leads. On this point of view, an experimental violation of the impotence postulate would not be interpreted as a complete overthrow of the whole theoretical structure of relativity, but merely as an indication that we were not dealing with inertial systems. Methodologically, I cannot help thinking that this is of considerable importance.

To summarize briefly the point of view maintained here, though principles of impotence or renunciation are interesting milestones in the development of physical thought, they are of too negative a character to serve as true signposts for future theories. It seems necessary to reiterate that physics will not flourish under the shibboleth *Nequimus* any more successfully than under the old discarded battle cry of the du Bois-Reymonds: *Ignorabimus*.

THE PARADOXICAL RIVERS OF THE GREAT PLAINS*

OREN F. EVANS

Professor of geology at the University of Oklahoma, Dr. Evans (Ph.D., Michigan, 1940) has been at Oklahoma since 1920. His summers he devotes to physiographic research and in other spare time writes both technical and semipopular scientific articles. In October 1945 we published his "Scientific Beachcombing."

IN THE early eighties a waggish New Yorker, recounting his experiences on a recent trip to the then frontier lands of Kansas and Oklahoma, described the region as a "land of more rivers and less water, more cows and less milk, and where [he] could see farther and see less than anywhere else in the world."

Today, should he repeat that journey across the Great Plains, he would find a landscape well dotted with towns and cities, and he would have no trouble in obtaining cream for his morning coffee—at a price. But he would find the rivers the same. They are now spanned by great bridges of steel and concrete, but they are still the same treacherous watercourses, full of quicksands and subject to sudden floods, that they were in the days when they took their toll of the wagon trains on the Santa Fe Trail. In the dry season, the channels of drifting sand lie flat and dreary under the brazen sky, their tediousness only accented by the occasional trickles of lukewarm water meandering between sluggish pools. But, if and when the rains come, these same sand-choked streams are changed in a few hours to raging torrents, and their flood waters fill the broad valleys from side to side, sometimes even turning the adjoining uplands into miry bogs.

To a casual observer, the Great Plains rivers seem to defy all known laws of normal stream behavior. Instead of cutting their valleys deeper, they are, throughout much of their courses, engaged in filling them by depositing more material than they take out. The beds of their tributaries, instead of being at a higher level than the main stream, often flow, over a great part of their courses, at a lower elevation. As a result, instead of the stream branches working away from the main stream into the neighboring drainage system, they are likely to turn back and commit piracy on the parent.

The main stream, instead of remaining at a nearly constant gradient between high and low stages, increases its slope rapidly as the floods rise. Again, a tributary often has a broad, flat bottom with the stream meandering through it in a deep, narrow trench. To the student of stream work, this suggests rejuvenation, but in the Great Plains streams it means nothing of the kind. In this environment, it is the perfectly normal development of a degrading watercourse. The explanation of such seeming anomalies lies in the climate, the rock structure, and the nature of the sediments that cover the Great Plains.

From the Missouri on the north to the Rio Grande on the south, the more important rivers of the Great Plains head in the Rocky Mountains or their foothills, and with a steep gradient and an abundant supply of clear water they move rapidly eastward, occasionally breaking into rippling rapids or foaming cascades. But their nature quickly changes when they leave the mountain fringes and begin their long journey across the plains.

In passing from the steep, well-watered slopes to the relatively dry, flat, sandy plains, their volume becomes less, they flow more slowly, their waters turn turbid with suspended sediment, and they no longer cut downward in their beds. For two hundred fifty miles they struggle eastward, losing water to the thirsty sands as fast as their tributaries can supply it and, in dry seasons, barely able to maintain their flow.

The annual rainfall of these High Plains averages between ten and twenty inches, is irregularly distributed, and varies greatly from year to year. Much of it comes in local torrential downpours that hurry great loads of sand down first one tributary and then another into the already overloaded channels of the main streams.

Farther east, toward the Mississippi River, the precipitation gradually increases, but not fast enough for the streams to take care of the increased loads of sediment. So, even here, they are often filling their valleys faster than the material can be carried away. When filling occurs in normal streams in humid regions, it is usually the result of a decreased gradient brought about by a general lowering of the land level. In these Great Plains streams, on the contrary, filling results from a load of sediment greater than the stream can carry.

* Drawings by R. L. Harris, Oklahoma Geological Survey.

In fact, for streams of their size, the rivers of the Great Plains have exceptionally steep slopes, varying from two to ten feet per mile.

These peculiar conditions are the result of an evolution that began in Cretaceous times. In middle Cretaceous an interior sea spread from the Arctic to the Gulf and reached east almost to Lake Superior. At that time there was no Mississippi Valley as we know it, and the drainage of the continental interior was very different from what it is today. Many of the streams flowed west into the Cretaceous sea, and the Mississippi was only a small stream emptying somewhere near the present city of Cairo, Illinois, into an embayment reaching north from the Gulf. It was then a very humble stream and gave little promise of developing into the huge drainage monopoly it has since become.

When the sediments that had been depositing for millions of years in the Cordilleran trough began to be squeezed up, however, the situation quickly changed. As the bottom of the trough came above the level of the sea, the whole area became a great swamp that remained for many thousands of years, during which it was covered with a succession of dense forests. These furnished the material for the great beds of peat that have since been transformed into the coal fields of the Rocky Mountains. In the beginning, the rise of the land was slow, but it became more rapid toward the end of the Cretaceous as it developed into the early Rocky Mountain uplift. This movement set up an entirely new drainage system for the interior of the continent. As the eastward slope increased, the swamp streams took on new life and their movements became more purposeful and definite as they flowed east and south toward the youthful Mississippi River and the Gulf.

At first, these streams were relatively short and their valleys shallow. But as time went on and the region to the west continued to get higher and higher, they began reaching headward toward the rising mountain axis. At the outset, the area was well watered and the streams developed normally, slowly lengthening as they cut their valleys deeper. As the surface by degrees took on this eastern tilt, the low gradient and the great volume of water, along with the level-lying rocks, was conducive to the formation of broad and relatively shallow stream valleys.

But there came a time when the rising mountains to the west began shutting off the moisture from the Pacific. The rainfall became less and less, the forests died out, and the region of the headwaters of the streams gradually took on the appearance of a desert.

By this time the streams, in their upper reaches, had cut down enough to begin to expose the Dakota sandstone that covers broad areas all along the east side of the Rockies. As more and more of it was uncovered, the tributaries rushed great quantities of sandy material into the streams and they began filling their valleys. The mountains to the west continued to rise, the climate became more arid, the rainfall more erratic, and the streams seemed to be flowing in valleys too large for them. This aspect has been accentuated with time and persists to the present day.

Another puzzling feature of the plains streams is in the peculiar relations existing between the large streams and their branches. In a normal degrading stream of the humid regions, a tributary, being younger and smaller than the main stream, runs at a higher elevation. But for a number of the older and longer tributaries of the plains streams, this relationship does not hold. At equal distances above the junction, the beds are often at a lower level than that of the main stream.

These seeming anomalies result from the way the drainage has developed. In its early history, while the rivers were still cutting their beds lower, they threw out numerous branches. Then, when the parent streams had cut down into the sandy rock beds of their upper reaches and began taking in a greater load than the water could carry away, they started to aggrade. But the tributaries, being shorter than the main streams, failed to reach into the sandy area and so had no opportunity to pick up the extra load. Instead of aggrading, they continued cutting their beds lower, as they had been doing from the first, with the result that many were soon flowing lower than the main stream. The topographic maps of the region show many such relationships.

Not far from the town of Norman, Oklahoma, Little River, a tributary of the South Canadian, is some fifty feet lower than the main stream. This is at a point some fifty miles above the junction of the two. Here one branch of Little River is cutting straight toward the South Canadian and is already within three miles of it (Fig. 1). It seems certain that within a few hundred years, the two will join. Then, in time of flood, the water from the larger stream will leave its present bed and go roaring down the Little River Valley. Thus Little River will have committed piracy on its parent, and the bed of the South Canadian will be dry except for the small amount of water that drains into it locally.

A similar condition exists a few miles to the north, near Oklahoma City. Here Deep Fork, only

a few miles from the North Canadian, is flowing some forty or fifty feet below it and is in a favorable position, if left to itself, to commit piracy on its parent. Yet the two streams flow ninety miles east before they come together. Again, near Tulsa, Oklahoma, there is a similar relationship between the Verdigris and the Arkansas, though in this instance the two streams are somewhat farther apart and piracy is not quite so imminent.

This diversion of the waters of a stream by one of its branches is believed to have happened fairly frequently in the past history of the region. From Byers, Oklahoma, to near Haileyville lies the Gerty sand, which has the general shape and appearance of being an old stream bed. It is about ninety miles long, up to fifty feet thick and, in places, as much as four miles wide. It is thought to mark a former course of the South Canadian River, which was diverted from its bed sometime in the Pleistocene through piracies committed on it by some if its tributaries. It is interesting that Little River, which at one time was considerably longer than now, was involved in the diversion.

Though Little River probably began its existence later than did the Canadian and other large streams of the plains, its valley has the shape, even well up among the small branches near its head, that is usually associated with an old rejuvenated stream. To a student of streams, the first sight of it is as startling as seeing a girl in her teens with gray hair. But the broad, flat terrace, with the deep channel winding through it, does not indicate old age. Here it is the normal appearance for a degrading stream. It results from the combined effects of the horizontal rock structure and the sudden intense floods succeeded by long periods of drought.

The horizontal rock layers, some soft and some hard, are conducive to the cutting of a wide valley. During heavy floods, water fills the valley from side to side, depositing over it a thick layer of alluvium and forming a broad, flat flood plain within which, during the long periods of low water, the stream flows in a fixed, narrow channel. Thus, though a steadily degrading stream, Little River has a cross section like that usually associated with old age, or with old age followed by rejuvenation.

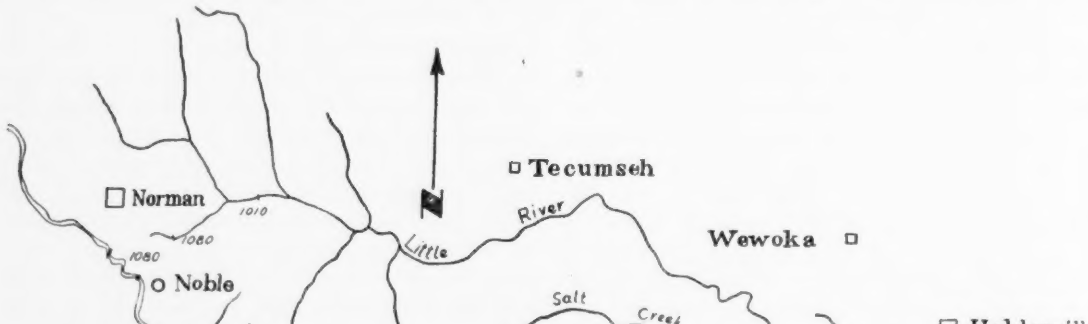


FIG. 1. THE SOUTH CANADIAN AND LITTLE RIVERS

SHOWING A CASE OF PIRACY IN THE MAKING JUST SOUTH OF NORMAN, OKLAHOMA.

During low water, the main streams of the plains, with their aggraded valleys, instead of flowing in deep, narrow channels, wander widely from side to side within their broad, shallow valleys. Because of the loose character of the material, they change their courses easily and often, with the curves working rapidly downstream. Such streams are very difficult to control.

In streams confined to rocky channels, the downstream travel of the bends is slow, but in plains streams, where the water wanders here and there between loose sandy banks, their courses change rapidly. Thus, a river flowing on one side of its valley today may, within a year, be found

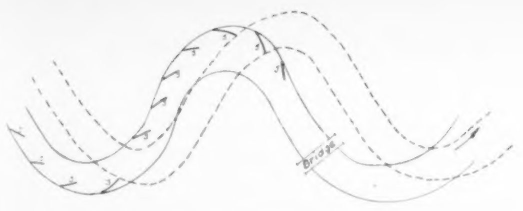


FIG. 2. A LOST BRIDGE

HOW THE CURVES OF A RIVER, BY MIGRATING DOWNSTREAM, MAY CAUSE THE ABANDONMENT OF A BRIDGE. *J* SHOWS HOW BY THE USE OF A NUMBER OF STRATEGICALLY LOCATED JETTIES SUCH A WANDERING STREAM COULD BE CONTROLLED.

a mile away on the other side. This situation is complicated by the fact that in time of flood the entire bed and valley may be full of water.

Such behavior makes bridging a difficult problem, especially so at a place where the river happens to be flowing down the middle of its valley. The bridge may no sooner be finished than there comes a flood, the curves in the stream shift downstream, and the river that was under the bridge is now flowing out around the end (Fig. 2). Something like this happened near Norman, Oklahoma, some twenty-five years ago. At that time there was an iron bridge about four miles southwest of town under which the water had flowed for eight or nine years. Then came two or three years of high water, during which the river curves moved rapidly downstream. This threw the channel from under the bridge near the north bank over close to the south side of the river, where it cut rapidly around the end of the bridge, and, before it was through, it was half a mile away to the south, having nearly doubled the width of its bed. For several years it continued to flow close against the south bank. But now, after some twenty years, the next phase of the river curve has reached the bridge, and the channel at low water is again back about where it was in 1920.

The difficulty of the bridging problem is much reduced where one side of the stream is against a high bank of rock. If this happens to be on the outside of a broad curve where the river is thrown hard up against the rocky shore, the bridge can be built in full confidence that it will be useful for many years. At such a location, a few miles above the Norman bridge, the Newcastle bridge has functioned continuously for forty years. But even in the absence of such a favorable location, it may be possible to keep the stream under the bridge by exercising constant vigilance.

It is a matter of persuasion rather than force. The control should be started some distance up the stream. With a good map or aerial photograph, a skilled physiographer can tell pretty closely the position the curves will assume as they migrate downstream. By the judicious use of wing dams and jetties on the first two or three curves above the bridge, the stream can be gently turned from its wandering ways and, in most cases, kept within its channel beneath the bridge.

The only other method of control is the building of heavy stone or concrete revetments and wing dams in the immediate vicinity of the bridge; but where there is no solid rock in which to anchor the outer ends of the structures, the method is very troublesome and expensive. It has been successfully done at some railroad river crossings, but for highway bridges it can seldom be justified.

For the geomorphologist, the streams of the Great Plains present many enticing problems. They obey the same laws of stream flow as do rivers in the humid regions. Yet, because of differences in environment, the results sometimes appear to be exactly opposite.

SHEFFIELD SCIENTIFIC SCHOOL—THE FIRST HUNDRED YEARS

CHARLES H. WARREN

Dean Emeritus Warren (Ph.D., Yale, 1899) was instructor of mineralogy at Sheffield during 1900–01. For the next twenty-one years he taught at M.I.T. In 1922 he was made Sterling professor of geology at Yale and dean of Sheffield. This article is from an address delivered at the Centennial Convocation in Woolsey Hall on October 17, 1947.

IT IS the aim of this article to give a summary history of the founding of the Sheffield Scientific School, of its development during its first century of life, and to point out the part that it played in the evolution of higher education in the American college or university.

The firmly established pattern of education in the American college up to the middle of the nineteenth century was of the classical academic type, inherited in large part from the ancient universities of England. Its broad objective was to educate young men for service in "Church and Civil State," and this objective was to be achieved by storing the student's mind with a thorough knowledge of the Greek and Latin languages and of the civilizations they recorded, to provide him with what was regarded as a sound and orthodox theology, a correct religious and moral philosophy, and to train his mind in logical thinking. As a part of the process he was exposed to a modest amount of mathematics and natural philosophy, which latter included some physics, astronomy, and natural history. There was no opportunity to study the sciences in any modern sense.

During the first half of the nineteenth century a great expansion was taking place in agriculture, commerce, and industry, and with this arose a demand for men trained in scientific and technical knowledge who could cope with the problems arising on every hand. Young men with practical interests were seeking an education that would equip them to meet this demand. The colleges offered them little or nothing of that sort. This fact was recognized by some of those interested in education, and as early as the 1820s here and there "Technical Institutes" were organized. These were patterned after similar schools abroad and, although they were of post-secondary school level, they were not of collegiate grade.* During the period roughly from 1850 to 1870, a considerable number of colleges took more or less definite steps to meet the call for scientific and engineering instruction, and there were also founded a few independent technical schools on the collegiate level.

It was a Yale president, Timothy Dwight the Elder, to whom I think should be given the credit for starting, indirectly at least, the scientific educational movement in the colleges on its way. As early as the turn of the last century this wise and learned man had reached the conclusion that science and its applications were soon to assume an importance that made it advisable for the college to give them a fuller recognition in its educational program. He had in some way persuaded the Yale Corporation to authorize the appointment of a professor of chemistry and natural history, and in 1802 Benjamin Silliman was appointed to this chair. This act, as subsequent events were to prove, was Dwight's contribution to the cause of scientific education.

Benjamin Silliman (B.A., Yale, 1796) was only twenty-three years old when appointed. He had been a tutor in the college and was but newly admitted to the bar; he had only the barest acquaintance with chemistry or any other science, but after two years' study of science, chiefly at the Medical School in Philadelphia, he assumed his duties at Yale in 1804. Dr. Dwight's ideas of the qualifications for a professor of chemistry seem a bit strange in these days, but it must be admitted that he was a shrewd and competent judge of men. He picked a great man.

During the next forty-odd years Silliman did more than any other man to arouse an interest in science in America. Endowed with a striking appearance and personality, with extraordinary gifts

* These are not to be confused with the trade school or with the junior college. They occupy a place between the secondary and trade schools and the engineering or technical college. Only a few of them have survived in their original form and few if any have been organized in recent times. I think this is unfortunate, for they might be made to serve an important function in our educational program, relieving our four-year, degree-granting institutions of truly collegiate grade of many students whose capacities and interests could be more effectively served by "Technical Institutes" of this type.

of mind and character, unexcelled as a lecturer, he preached his gospel of science far and wide throughout the country, always insisting that scientific studies were nothing less than an inquiry into the manifestations of the works of God in the natural world and that a wise use of the knowledge so gained would result in great benefit to mankind. He prepared the way for the acceptance of the study of science as a desirable and essential part of a liberal education.

Although Silliman's lectures in the College in chemistry, and later in geology and mineralogy, were notable and very popular with students, they were open only as electives, with little or no academic credit. However, he attracted a number of private students, most of whom later became leaders in scientific work and education. As their numbers increased, he felt the importance of extending the scope of this work, of adding new teachers, and of providing facilities for laboratory and experimental work, then practically nonexistent.

Silliman was responsible for the appointment, in 1846, of two professors, John Pitkin Norton, in agricultural chemistry, and Benjamin Silliman, Jr., in practical chemistry. In the same year a committee nominally headed by President Day, but actually by Benjamin Silliman, was formed to consider the advisability of establishing a distinct department of the College within which courses of study should be offered which were, or ought to be, given for others than members of the undergraduate classes and which were not in the Departments of Theology, Law, or Medicine. A favorable report was made by this committee and on August 19, 1847, the Corporation established a Department of Philosophy and the Arts to perform the functions designated by the committee. Under this department instruction was to be offered in the sciences and other subjects to graduates of the College and to other students of good moral character and adequate preparation. The creation of this department was the initial step in establishing postgraduate education in the American college, and presently, in 1859, on the recommendation of the faculty of the Scientific School, the Corporation established the degree of Civil Engineer and in 1860 that of Doctor of Philosophy. This latter degree was first conferred in 1861 on three candidates, to be followed two years later by a conferral of the degree on a man of the highest renown, Josiah Willard Gibbs.

The creation of this department also paved the way for the development of a new type of undergraduate education exemplified in the courses of instruction soon to be offered by the Sheffield Scientific School. It was the beginning, not only of the future Scientific School, but also of postgraduate education in America. It was an important step in the evolution of our university education.

The appointment of John P. Norton and Benjamin Silliman, Jr., both chemists, with assignment to this new department, led naturally to the offering of courses in that field as the first scientific courses in this department. This work was very soon being referred to as a "School of Applied Chemistry." In 1850 the Corporation, upon the recommendation of Silliman, John P. Norton, and James Dwight Dana, instituted the degree of Bachelor of Philosophy, and it was first conferred in 1852 on eight students who had completed the then two years' program of study.

In the same year, acting upon a recommendation of the men just mentioned, William A. Norton, an engineer, educated at West Point and then teaching at Brown University, was appointed as the first professor of civil engineering at Yale. He immediately organized a course of study in engineering. This was referred to as "The Yale Engineering School." In 1853 these schools of chemistry and engineering were together unofficially designated "The Yale Scientific School."

It was at this time that John P. Norton, broken down by overwork, died at the age of thirty. He it was who during the five years of his brief service carried the main burden and responsibility of teaching and formulated many of those policies of scientific study and research that were in large part to form the basic pattern of education in the Scientific School. Norton was the founder of agricultural science in America, and as a result of the training and inspiration received from him several of his students, John Addison Porter, his successor, William H. Brewer, and Samuel W. Johnson, all members of the School's first faculty, carried on his work. Indeed, this group was largely responsible for the designation of the School by the state of Connecticut to serve as its College of Agriculture and the Mechanical Arts from 1863 to 1895 under the Land-Grant College Act. One of this group, Johnson, in addition to his important contributions to the science of crops and soils, has the distinction of having established the Connecticut Agricultural Experiment Station, the first in the country and still functioning as one of the foremost institutions of its kind.

The small but distinguished group of men who made up the early faculty of the School found themselves faced with the problems of providing

adequate instruction for a steadily increasing number of students and of broadening the scope of the School's work. The funds available, derived from the meager tuition fees and from contributions by members of the staff itself, were all they had. Outside support must be sought—how familiar that sounds!

On August 19, 1856, James Dwight Dana, Silliman's successor as professor of geology and mineralogy and America's foremost exponent of these sciences, gave the Yale Commencement address and made it the occasion of an eloquent appeal to the alumni and friends of Yale to support a plan for a better and more extensive organization of scientific work and instruction in the form of a distinct Scientific School. He gave a comprehensive outline of this plan, which presented a remarkable vision of the place of science in a university. This address was published, as were two other pamphlets written on the subject under his inspiration. These were widely circulated and excited much interest. They are well worth reading today.

The response to these appeals was immediate. A number of individuals gave substantial sums, but the principal donor was Joseph Earl Sheffield, of New Haven, railroad builder and financier, who was deeply and intelligently interested in promoting scientific and technical education. He not only gave the School substantial sums of money but a building, which he caused to be remodeled, enlarged, and well equipped for its use. This building (known as Sheffield Hall), now gone, stood at the head of College Street and was, I believe, the first college building to be devoted exclusively to scientific work. In recognition of Mr. Sheffield's generous support, the Corporation in 1861 named the new school "The Sheffield Scientific School." He continued his benefactions during his lifetime and was responsible for the formation of a Corporation, known as The Board of Trustees of the Sheffield Scientific School, to hold funds and property for the use of the School. In his will he made the School through this Board one of his heirs and his residuary legatee.

The success and prestige of the School were due principally to the extraordinary group of men who presided over its affairs during its earlier decades. They were eminent as scholars and teachers, able administrators, and pioneers in establishing a new type of education. They gave a self-sacrificing devotion to this cause and in the end established a pattern of liberal scientific education which influenced strongly the whole trend of higher education in the American college. It may be added that the condescending disapproval and suspicion with which the academic faculty chose to regard the alleged vocational training offered by the Scientific School undoubtedly acted as an additional spur to its faculty to justify and promote its program of education, which it firmly believed was a sound and liberal one. Indeed, there is evidence that there was often a good deal of—shall we say?—vigorous argument between the two groups as to the relative virtues of their divergent views on education.

I have already mentioned some of the early faculty members. It is not possible to give here an account of those who were active during the early decades nor of their many distinguished successors in the fields of science, engineering, and the humanities in later years. A full account of them may be found in Director Chittenden's admirable and comprehensive *History of the Sheffield Scientific School* (New Haven: Yale Univ. Press, 1938). I shall confine myself to mentioning only a few names, in addition to those earlier referred to, of those who seem to me to have made some of the more significant contributions to the School's development and whose names also illustrate the breadth of the School's program. I must mention George Jarvis Brush, mineralogist, the first director of the School and its treasurer; Russell H. Chittenden, the founder of physiological chemistry in this country, the originator of the first organized program of study in subjects preparatory to the study of medicine and the medical sciences, and the successor in 1898 to Dr. Brush as director of the School. To these two men, who guided its destinies for well over fifty years, the School owes an inextinguishable debt of gratitude. I will mention also William P. Trowbridge, the first professor of dynamical or, as we call it, mechanical engineering, and the organizer of the first course of study in that field; General Francis Amasa Walker, the eminent economist, who later became president of the Massachusetts Institute of Technology; William D. Whitney, the most distinguished linguistic scholar of his generation; Thomas R. Lounsbury, noted English scholar and writer and brilliant teacher, who effected a revolutionary change in the teaching of English literature; Chester S. Lyman, professor of astronomy and physics; Othniel C. Marsh, eminent vertebrate paleontologist; Addison E. Verrill, zoologist; Daniel Cady Eaton, botanist; and, last, Daniel Coit Gilman, professor of physical and political geography. I mention him particularly because I do not think that the importance of his connection with the School both to it and to Gilman himself has been adequately appreciated. His

remarkable and versatile abilities, as well as his extensive knowledge of educational practices here and abroad, were fully recognized by members of the Governing Board. He became its official spokesman. His articles and addresses regarding the School became, as has been said of them, "The School's Articles of Faith" and have seldom been equalled and never excelled as statements of sound educational principles. It was during the nine years of his connection with the School that much of the educational philosophy was crystallized that later found a brilliant and epoch-making expression in his accomplishments as president of the Johns Hopkins University.

Years later, in an address Gilman gave at the fiftieth anniversary of the School, he said, in part:

> In quick succession, colleges, departments of science and independent institutions have appeared in every state. Of these not a few have adopted the methods here followed or have called to their support those who have been trained here. For one such institution, now celebrating its majority, permit me to acknowledge with filial gratitude the impulses, lessons, warnings, and encouragements derived from the Sheffield Scientific School, and publicly admit that much of the health and strength of the Johns Hopkins University is due to early and repeated draughts upon the life-giving springs of New Haven.

I TURN now to a brief review of the School's actual program of instruction. It was at first conducted on the postgraduate level, to which was soon added a three-year undergraduate program. Both expanded rapidly in numbers and scope and were administered together until 1918–19, when all postgraduate instruction was transferred to a separate Graduate School. I need not dwell on the character of the early postgraduate program since it differed little in principle from prevailing university practice.

In the undergraduate program, with one exception, the courses offered were confined to the major fields of science and engineering. The objective of these courses, clearly stated from the beginning and adhered to consistently, was that of providing a foundation in the basic principles, concepts and methods of scientific work, the mastery of which must precede further advanced study or successful practical applications. These studies were to be accompanied by a substantial amount of attention to English, the modern languages, and other cultural and humanistic subjects. In other words, a liberal education with the emphasis on science. As scientific and technical knowledge increased, the basic courses were broadened in scope and new subjects and new major courses were added.

There was one course, however, of a radically different character, the celebrated "Select" course, now but a memory. There was on the part of influential members of the faculty a feeling that the School in breaking away from the traditional classical curriculum ought to champion the idea that a liberal education could be secured through pursuing a course made up of the modern humanities, English, modern languages, the basic sciences, and mathematics, and as early as 1860–61 a course in "General Studies" was offered. In 1864 this course was reorganized, under the influence of Gilman, and an excellent selection of required subjects was offered under the title "Select Course in Scientific and Literary Studies." It was a unique course and undoubtedly contributed to the development of modern college curricula in the liberal arts and sciences. It had a strong appeal to many students and grew rapidly in numbers until it became by the end of the first decade of this century the largest course in the School; it even overshadowed to some extent the scientific and engineering courses. Whatever its shortcomings may have been, it was a good course in its day and graduated many men prominent in afterlife. It came, in time, to be out of place in a university that also maintained another liberal arts course in the academic college.

Yale College, up to about 1880, had adhered closely to its traditional curriculum. It was opposed to any substantial change and felt no responsibility for education in the sciences. It was content to leave that to the Scientific School. There was, however, a faculty group, led by William Graham Sumner, who were strongly in favor of liberalizing and broadening the curriculum to meet a growing public demand for such a change and to keep abreast of similar movements in other colleges. In 1884 Sumner set forth his views on higher education in a notable article published in the *Princeton Review*, entitled "Our Colleges before the Country." In this and in other essays referring to educational matters, Sumner's statements are remarkable for their breadth of view, force, and clarity. His views were in close agreement with, and supplemented, those expressed so well in an earlier day by Dana and Gilman, and all are well worth reading and pondering in these days when the educational world is all astir with plans for reforming our educational program. I have yet to hear or read anything that, in so far as fundamental and enduring principles of education are concerned, was not fully covered by these gentlemen half a century or more ago.

The progress of Sumner and his sympathizers was slow. But they did succeed in imposing a more

liberal policy on the College, and, although this did not go as far as Sumner desired, it opened the way for future constructive changes. Gradually, departments and laboratories of chemistry, physics, geology, and biology were established in Yale College, and programs of study including these sciences were offered. As a result, a large amount of duplication developed between the two schools in the sciences and even in the humanities. This situation as a whole led in 1918–19 to a reorganization of the University, which effected many important changes. It established University departments of study, each to serve the needs of the several schools. It placed all postgraduate study under a Graduate School. The three-year course of the Scientific School was made a four-year course. It abolished the "Select Course," as well as a graduate program in Business Administration that had been started in the Scientific School with a substantial endowment. It established a separate faculty to administer a program of freshman studies preparatory to the courses of both Yale College and the Scientific School. The Scientific School then became a purely undergraduate school for the study, it was stated, of professional science and engineering; Yale College was to be the school for the study of the liberal arts and sciences, with emphasis on the humanities. The engineering courses could be regarded as professional in character, but the attempted distinction was not valid for the science majors offered by the Scientific School. The science programs in the latter were more rigorous and definite in character than those of the College, but there was nothing to prevent a student in the College, majoring in the sciences when working for his B.A. or Ph.B. degree, from following substantially the same programs as those offered in the Scientific School leading to the B.S. degree. He could study the same subjects and under the same teachers, and many were permitted to do so. The choice of school by the student who wished to major in a science became largely a matter of which degree he preferred and which school with its distinctive social system appealed to him most.

In 1932–33 the residential college plan, made possible by the generous gifts of Mr. Edward S. Harkness, was inaugurated. This brought upperclass undergraduates together as residents of the colleges, with admirable opportunities for intellectual and social intercourse, not only with each other but with the faculty Fellows of each college. It tended to obliterate that unique Yale practice of dividing its undergraduates into the "Ac" and "Sheff" species according to their school affiliation, a division that once had a real significance but had become largely a survival.

The engineering departments, which had originated and grown up as an important part of the Scientific School, but which had been constituted University departments with their instructional duties divided between the Graduate and the Scientific Schools, were united in 1932–33 to form a distinct School of Engineering, thus conforming to the general practice of American universities.

During the war years just past a committee representative of the many fields of study, was appointed by Dean William C. DeVane, of Yale College, to make a thorough study of undergraduate curricula and to draw up a new program better adapted to the objective of educating our young men for service in the postwar world. When this new and admirable program was completed, it became clear that it incorporated, among other desirable features, the objectives, set forth in principle so well by Gilman, to which the Scientific School had always adhered. This new program was such that the science courses of the Scientific School could with only minor changes be incorporated into it, and it seemed eminently wise on all counts that all undergraduate instruction should henceforth be administered by a single united faculty representative of the arts and sciences. This was accomplished in 1945, and Yale now offers to all its undergraduates, except those in the Engineering School, under the faculty of Yale College, a well-balanced, fully integrated, yet flexible, program of study designed to provide its students with a knowledge of the important fields of human thought and accomplishment, together with an opportunity to concentrate and master the fundamentals of some field of study chosen according to their individual aptitudes and interests, and, most important of all, to help them to secure a mental discipline and outlook that shall help them to use their education to the best advantage.

You may now ask what is to be the function of the Sheffield Scientific School as it begins its second century. The old "Sheff," as men of my college generation knew it, has disappeared. It has now resumed its original function, that of postgraduate education, and has added certain new and appropriate functions that will enable it to preserve its integrity as an institution, its honorable name, carry out the purpose for which it was founded, and move forward to new achievements as a vital part of the distinguished University which gave it birth and which it has served so long and so well.

The faculty of the School will henceforth

concerned with the instruction and guidance of students registered in the School, candidates for the degrees of Master of Science and Doctor of Philosophy, and with scientific research. The Board of Permanent Officers of the School has been constituted the University Division of the Natural and Physical Sciences and Mathematics and as such will, under the President and Corporation, exercise general supervision over educational policies for all study and research in these fields, including particularly the correlation and integration of teaching and research in the departments within the Division. Most important of all, the Director and his Permanent Officers, acting through standing or special committees, will examine and pass judgment on all recommendations for promotion and new appointments to the faculties of the departments within the Division and report the decisions to the departments and schools to which they may be assigned, and to the President and Corporation for their information and for final action. Thus, all scientific work in the University will be under the general supervision of a single competent group of scientists. This arrangement restores to the School functions that it exercised so successfully during the earlier decades of its history.

The Sheffield Scientific School will go forward into its second century of service with all its energies and resources devoted to the pursuit of its time-honored objective: "The Promotion of the Study of the Natural, Physical and Mathematical Sciences"—to discover and interpret the laws and phenomena of the natural world to the end that these may be wisely and humanely applied for the welfare of mankind.

PARADOX

Not truth, nor certainty. These I forswore
In my novitiate, as young men called
To holy orders must abjure the world.
'If . . . , then . . . ,' *this only I assert;*
And my successes are but pretty chains
Linking twin doubts, for it is vain to ask
If what I postulate be justified,
Or what I prove possess the stamp of fact.

Yet bridges stand, and men no longer crawl
In two dimensions. And such triumphs stem
In no small measure from the power this game,
Played with the thrice-attenuated shades
Of things, has over their originals.
How frail the wand, but how profound the spell!

CLARENCE R. WYLIE JR.

SCIENCE ON THE MARCH

TEAMWORK IN THE SOCIAL SCIENCES*

IN THIS article I shall seek, first of all, to test the hypothesis that a greater part of the conflict among contemporary scientists is methodological in nature and that these methodological controversies are largely the result of the reactions of individual scientists to scientific traditions. Second, I shall offer certain suggestions for resolving scientific controversies and for promoting cooperative endeavors in the social sciences.

If we could all agree upon some definition of science, it would have to be a simple one, such as: Science is tested knowledge. Or, science is all endeavor to ascertain facts and their interrelations. According to these definitions, each science may determine what knowledge it should test or what facts and their interrelations it should ascertain. Furthermore, such definitions place no limitations upon any science as to the methods and techniques it should employ in research. This spirit of freedom is one of the most important of our scientific traditions. Of course, it is broken occasionally by political coercion, as was the case in Hitler's Germany; it is also limited by the subject matter that may be accepted as belonging to each discipline, limited somewhat by the leaders in the various scientific fields, and by the pressures from society and culture, which are controlling forces in human activity or achievement.

Although this spirit of freedom is regarded as essential for scientific progress, it may retard such progress where individual scientists spend their time and talents carrying on heated controversies as to the accuracy and validity of the work of others. Too often, perhaps, one scientist judges the work of another in terms of what he thinks the other person *ought* to do, *should* have done, *should not* do, or what he *ought not* to have done. In many instances, this "ought attitude" is carried over into the future tense to express a sort of "scientific idealism." Hence, the tradition of scientific freedom tends to motivate personal controversies. The personal patterns of such controversies are intensified when certain theories are criticized, amended, refuted, or exposed as erroneous.

A second tradition handed down to modern scientists is that some of the sciences are "pure," some are "applied," and others are neither, but really belong in the field of technology or art. Much of the controversy over this question has been suggestive and stimulating both from the standpoint of the classification of the various disciplines and from that of modifying and standardizing scientific method, but it has often led to dogmatic conclusions that have set up barriers to scientific research, on the one hand, and, on the other hand, has produced a pattern of unhealthy autonomy among some of the sciences.

This sort of scientific dogmatism has sometimes resulted in an uncritical approach in scientific research, based upon the assumption that in this or that specific field of research the scientist is not simply courting objectivity, but is wedded to it.

It is my contention that such a tradition is misleading, and unwarranted in view of the facts; for no science is pure in the sense that it is wholly separated from applied science, technology, and art; every science is to some extent a pure and an applied science and at the same time something of a technology and an art.

Pure science asks the question "What is it?" and seeks to answer it by some system of research using a skill that involves technique and art. Applied science asks: "What can you do about it? What predictions and controls are suggested?" and, whether a particular scientist admits it or not, he is interested in predictions and controls, and in some respects is influenced in his research by these applied aspects of his science. Technology asks the question "How can you do it?" which involves the choice of equipment, techniques, and methods of research to be used. Art asks the question "How can we acquire skill in doing it?" which involves the development and application of certain skills for carrying on specific experiments and research.

Another scientific tradition almost universally accepted among scientists is the research process necessary for acquiring and testing knowledge. We say, first, that one must begin with the formulation of some problem; second, we give a tentative answer to the problem in the form of a hypothesis; then we collect data to test the hypothesis or to formulate new hypotheses; next we classify

* From an address on "Some Personal and Traditional Aspects of the Methodological Conflicts Among Contemporary Scientists" before the twelfth annual meeting of the Florida Academy of Science, Tallahassee, April 21, 1948.

and compare the data; then we interpret the findings; and in the final step before publication we verify the conclusions by repeating the process as many times as the researcher considers necessary to substantiate his conclusions.

It is doubtful, at the present stage of the development of science, whether this research process, as a general policy, can be improved upon. Yet there may be some question as to the ways individual scientists utilize methods and techniques in carrying on their research. Here is another source of conflict that is basically methodological.

The personal aspects of methodological conflicts in science are generally linked with convictions regarding scientific dogmas. I have selected five commonly accepted dogmas which tend to promote conflict:

1. The scientist separates his subjective preferences, emotions, and desires from the logical, universal elements in his thought and works completely objectively.
2. Scientific research is divorced from all types of biases, value judgments, and preconceptions.
3. Science employs universally valid and understandable symbols.
4. Quantitative symbols and language are scientific, whereas qualitative language and descriptions are vague and pseudoscientific, if not unscientific.
5. The subjectivity of qualitative descriptions can be objectified by transferring qualitative language into statistical and quantitative formulas and symbols.

Other personal aspects of methodological conflicts grow out of research methods and techniques employed by individual students. This is particularly true of observation techniques, the goals of research, the methods of logic employed, and the adaptation of quantitative techniques to the classification and comparison of data. Individual choices must be made constantly in each of these areas of research; hence, research tends to become personalized with each choice. But there is always the possibility that another person, with a different point of view or a different background, might make different choices. This personalized aspect of research not only leads to conclusions that often conflict where two or more individuals are studying the same problem, but it also makes for methodological conflicts as well.

Scientists are fairly well stratified into groups in terms of their acceptance or rejection of certain traditions, dogmas, and fads of science. Such stratification is often stimulating, but it is likely to produce numerous methodological conflicts. For example, there are those scientists who stand by scientific traditions as opposed to those who feel that problems of values and ends should and do come within the scope of scientific methods. Likewise, there are those who feel that there is essentially no difference between the methods of the physical and social sciences, as contrasted with those who forcefully seek to draw lines of methodological distinction between the two.

In every case, the conflicting positions are essentially methodological, and they grow out of the personal reactions of individual scientists to scientific tradition, on the one hand, and to the attitudes and goals that the respective scientists hold for themselves or for fellow-scientists, on the other. Our most important task at present is to develop a cooperative spirit while coordinating the efforts of all individual scientists and all groups of scientists.

The physical sciences have demonstrated their capacity in such a manner that they are revolutionizing surgical, medical, and related practices. They have gained the respect of a greater part of the civilized world. For the most part, the social sciences have not gained this type of respect. Principles and laws of human relations, as developed by the social sciences, have not been accepted with the authority many of us feel they deserve.

We have reached a point in the development of scientific thought and research, and in the development of civilization as well, when both social and physical scientists must reconsider their roles in the life of man. The scientific age in which we live would seem to preclude drastic competition and conflict between the sciences as well as the complete autonomy of any science, or any group of sciences. But since the competition and conflict now existing among scientists rest so largely upon the problems of methodology, the immediate task before us is that of developing a uniform methodological outlook that is at once critical and constructive. This would tend to weaken the forces that are causing some lags in our scientific knowledge, while reducing the conflicts within the scientific realm itself. When this has been accomplished then the physical and social sciences can move toward reducing those external forces such as establish intolerance, institutionalized suppression, and selfish interests of all types, which too often impede scientific progress by producing confusion, strife, and conflicts among individuals, groups, and nations.

I do not mean by this that science can save the world or solve all its problems, but it will be able to contribute more in this regard through cooperative scientific effort than it is now able to do with scientists often working at cross-purposes or in actual conflict with one another. But not all our

methodological conflicts need be resolved before cooperation begins.

One cooperative effort urgently needed is that of organizing the research of social scientists in such a manner that we can bring together, in a well-planned, systematic, and synthetic summary, the contributions social scientists have admittedly made in the fields of community planning, juvenile delinquency, housing, marriage and family life, international relations, education, sanitation, health, and the like. Such a venture would not only help the teachers of social science by placing in their hands valuable teaching aids, but it would also stimulate the application of certain fundamental social principles and laws, which need social sanction in the same sense in which certain discoveries in the physical sciences have been applied in solving individual and social problems.

Teamwork is especially needed in the development of original research in the social sciences. There are at least five ways in which cooperative research may be promoted at present:

Coordination in planning research. This involves pooling research interests and making them known through publication of research projects in process. This is already being done by various professional societies. But the planning should go beyond this and designate those students who are available for collaboration in given fields.

Cooperative conferences in promoting research. This would involve bringing together students from a particular locality or those with common research interests to discuss and plan research programs and methods and to allocate the work agreed upon by those participating in the project.

Cooperative utilization of resources and skills. Short institutes might be held for specific types of research training after a project is begun. These institutes would resemble the so-called workshop training courses, where all individuals exchange information and discuss various types of skills and samplings of research already in progress. They might also lead to cooperative use of research equipment and trained technicians.

Teamwork in the execution of research. More comprehensive studies might be made by following the symposium technique of farming out different aspects of a question to specialists, or by parallel studies of the same problem by different students in different localities using the same hypotheses and the same methods and techniques of research, or simply by joint participation in a limited field or in a specific locality.

Cooperative dissemination of facts and information. Although this type of cooperation does not fall strictly within the area of scientific research, it is of utmost importance. It involves every sort of educational skill, from the most technical and academic type of report and lecture to the most popular type of writing and speaking. The dissemination of facts should be directed to all groups and classes of people.

The social sciences have a long way to go if the fruit of those who labor in the field is to influence public opinion in ways that will benefit all members of society. There are numerous difficulties to overcome before the work of social scientists is accepted on the same footing as the findings of many of our physical scientists are already accepted. The gap between the two fields of science must be made narrower, and it appears that nothing short of cooperative research will give the social sciences the status they need to deal constructively with today's problems. Long before this goal can be realized, a united front among scientists must be developed. Teamwork, and teamwork alone, will eliminate many of the methodological conflicts between social scientists and lessen the controversies that now prevail between physical and social scientists.

Science has never been faced with a greater challenge than it has today. But science cannot meet this challenge unless we, as individual scientists, join hands with our colleagues and work diligently in an effort to resolve personal disagreements and controversies and strive to promote a spirit of tolerance, understanding, and cooperation. This is our challenge; let us continue to face it!

MELVIN J. WILLIAMS

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Hg^{198} AS A WAVE-LENGTH STANDARD

THE alchemist's dream of transmutation has come true, and, following the well-known rule that dreams go by contraries, the transmutation is not as he hoped, from base to noble, but from noble gold to base mercury. As an accomplishment of theoretical science, the new mercury in the form of a pure isotope is important, but some of this mercury in electrodeless lamps made by Dr. William F. Meggers, of the Bureau of Standards, may soon have a profound effect on our statistical data on wave lengths and thus be of immediate practical use. To understand some of the implications, it will be useful to recall part of the history of the accurate measurement of wave length, and how the hidden complexity of some seemingly simple spectrum lines has presented real barriers to progress.

When Michelson made his famous count of the number of wave lengths of the cadmium red line in one meter, he was under a severe handicap that was not fully understood until the existence of isotopes was discovered. Michelson, and others, were aware that different lines acted differently when used in his interferometer. Some would give fairly sharp patterns for short separations of the

mirrors, but at increased separations the concentric rings of light merged together so that they could no longer be observed and accurately counted. Other lines gave slightly better results, and cadmium 6438 was finally selected as being best suited or, more accurately, as least defective. Later, when the Fabry-Perot interferometer had been developed, the superior brightness and definition of the rings made wave-length measurements easier and more accurate. But no spectroscopist is ever satisfied with the last digit in a wave length, and it seems there must always be a last digit to annoy him. As the last digit moves to the right, however, it becomes less and less annoying, though not necessarily less important.

The green line of mercury, 5461Å, on first inspection looks like a single sharply defined line, well separated from its spectrum neighbors, and lying in the middle of the visible spectrum where the luminosity is high. It is thus ideal in all respects except one—it is really not a single line. The Fabry-Perot interferometer is an excellent detector for discovering the fine structure of spectrum lines, and this very resolving power interfered with the measurement of precise wave length. The development of the theory of isotopes helped explain the character of 5461 by pointing out that the 10 isotopes of mercury would give 10 lines grouped near 5461 and that the accurate designation of the true center of the group was practically impossible. The red line of cadmium also has a fine structure of about 12 lines, and it is only by coincidence that the arrangement makes the whole group better suited for precision wave-length measurement.

The answer to this difficulty would be an ideal source that is composed of a single isotope giving a complete spectrum of truly single lines. Meggers has constructed lamps containing a single isotope of mercury, Hg^{198}, and he now has available a spectrum of some 33 lines, each superior in inherent sharpness to anything previously obtainable. His abstract of the paper presented to the March 1948 meeting of the Optical Society of America reads:

> Electrodeless lamps containing five milligrams of Hg^{198} and five millimeters on a mercury pressure gage of argon excited by 100-megacycle frequency are found to be highly satisfactory sources of exceedingly sharp spectral lines. Employing Fabry-Perot interferometers and prism spectrographs, preliminary values of the wavelengths of thirty-three lines (2537A to 6907A) have been determined relative to the red radiation from cadmium simultaneously by imaging one lamp in the other. Interference patterns of the stronger Hg lines can be photographed in less than one second. Etalons* of 5, 25, 40, 50, and 67 millimeters have been used thus far. The probable errors of these preliminary wavelengths emitted by mercury 198 range from one part in twenty millions to one part in one hundred millions. The relative values of these wavelengths will be refined by measuring them in vacuum.

The attainment of this ideal radiator will probably lead to a resurvey of our entire literature of wave lengths, and perhaps a redefinition of the length of the meter, probably in terms of the mercury line 5461 that has in times past been so disappointing to physicists.

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* *Etalons.* A pair of mirrors permanently spaced a known number (g) of wave lengths apart. By moving the etalons through their own length, the wave lengths can be counted in groups of g, thus greatly shortening the work of counting.

ITINERARY*

FIRST ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

AUBURN CAMERA CLUB — June 28–July 12
Bureau of Animal Industry
U.S.D.A.
Auburn, Alabama

AMERICAN OSTEOPATHIC ASSOCIATION ANNUAL CONVENTION — July 19–23
Boston, Massachusetts

GENERAL FOODS CORPORATION — August 5–25
Central Laboratories
Hoboken, New Jersey

ACADEMY OF SCIENCE — October 1–15
Illinois Wesleyan University
Bloomington

* Dates available for showing 1948 Salon pictures.

BOOK REVIEWS

UNDERSTANDING SCIENCE THROUGH ITS HISTORY

Emanuel Swedenborg, Scientist and Mystic. Signe Toksvig. 389 pp. $5.00. Yale Univ. Press. New Haven, Conn.

THE strange and fascinating career of Emanuel Swedenborg (1688–1772) continues to baffle both the scientist and the theologian. He was one of those "universal" scholars that adorned earlier centuries. He knew astronomy, physics, chemistry, physiology, psychology, anatomy, mathematics, Latin, Greek, and Hebrew. He was a mineralogist, a builder of bridges and canals, an authority on mines and metals, a mechanical genius, a poet, a member of Parliament, a writer on good government and finance, and he traveled extensively throughout Western Europe. Some of his conclusions, such as the definition of matter as motion, his researches into the nature of blood, heart, brain, and the nervous system, and his delving into the realm of the subconscious mind, anticipated some of the achievements of present-day scientists and still have value.

Fortunately, or unfortunately, depending on one's interest in mysticism, Swedenborg's mind was not satisfied until it invaded that unseen world whose realities are not measurable by the instruments of science. He knew that "there are marvellous things occurring in the human mind, so marvellous indeed that they cannot be expressed," and he could not let his mind alone. He craved order and a realm of law in both the physical and the spiritual world, and he wanted to reconcile religion with reason and science, without offending either. Whether he succeeded, or went off the deep end, is still a matter of debate.

Swedenborg was engaged in a great quest for the soul, the formative force which causes the body, the immaterial which builds the material. He believed in a God who was the source of all living matter, and in his "mission" to cleanse all existing religions and introduce the new religion of the New Jerusalem based on a gospel dictated by the angels themselves. He entered the realm of mysticism by techniques not unlike those of Yogi; he had "God-intoxicated" dreams and visions, saw the divine flame of the mystic, and finally had the afterworld opened to him to such an extent that his spirit could commute freely among the planets and interview the characters of history from the Apostles to Newton and Louis XIV. As a result, he could describe heaven and hell in minute detail.

Swedenborg's literary productivity was enormous and phenomenal. Most of his volumes deal with his fantastic excursions into religious mysticism, and some are marked by the strangest symbolism and revelations. As a scientist he heaved "chunks of science" "into mystic joy." Many people have tried to find rational explanations for this dual personality. Some have talked about the sex suppression of an oversexed man; others have suggested paranoia and schizophrenia; still others have made comparisons with the telepathy and precognition of modern psychical research. The enigma and the strange duality of this exceptional person remain. Some of his writings are "scholastic sawdust" and incredible fantasy; others reveal a rich beauty and ethical content, and shrewd scientific observation on an amazing number of natural phenomena and human experiences.

One cannot do justice to so complex a character and so learned a biography in so short a review. If the great Swede remains an incomprehensible figure to this reviewer, who is neither a scientist nor a mystic, the fault is not the author's, for she has done a prodigious amount of research, has struggled sincerely, sympathetically, and with scientific objectivity with Swedenborg's many-sided career and his "revelations" and "systems," and she has produced the best analysis available.

CARL WITTKE

The College of Arts and Sciences
Oberlin College

The Beginnings of Modern Science. Holmes Boynton, Ed. xv + 634 pp. $2.39. Classics Club. New York.

THE book under review, although a scientific source book, differs from other publications of this character in that the latter are usually intended as reference books for the scientific specialist, whereas this volume is planned to give the general reader with an interest in the subject a better understanding of modern science. It contains writings of

the sixteenth, seventeenth, and eighteenth centuries, covering the general subjects of physics, chemistry, geology, biology, medicine, and scientific philosophy. In each division the plan has been to choose reports on related topics in order to show step by step the growth of thought in each subject and the improvement of experimental technique. Each division of this book is prefaced by an introduction giving a brief summary of its contents, and explanatory footnotes are given where the original article might be difficult for a modern general reader to understand. The original articles, of course, have been in some cases considerably abbreviated, but this has been done without disturbing the logic of the writing. This plan makes the book as a whole quite easy reading.

As an example of the articles contained in this book, the section entitled Matter and Motion includes, among others, Tycho Brahe's *On a New Star,* Cavendish's *Experiments to Determine the Density of the Earth,* and extracts from Newton's *Principia.* In the section on Electricity we find Franklin's description of his famous kite experiment, and *The Discovery of Current Electricity,* by Galvani. The section How Plants Grow includes *The Breathing of Plants,* by Ingenhousz, and *A Dissertation on the Sexes of Plants,* by Linnaeus. In The Science of Healing we find *The Invention of Vaccination,* by Jenner, and *The Prevention and Treatment of Scurvy,* by Captain Cook. On the whole, the selection of the articles has been well done, and the book can be recommended to the class of readers for which it was intended.

PAUL R. HEYL

Washington, D. C.

Christian Huygens and the Development of Science in the Seventeenth Century. A. E. Bell. 220 pp. Illus. $4.50. Longmans, Green. New York. Edward Arnold. London.

IN THIS, the American edition of a book first published in London in 1947, the author, who is head of the Science Department at Clifton College, gives an interesting account of the life and scientific significance of that great seventeenth-century genius Christian Huygens. The book is appropriately divided into two parts, the first being more in the nature of a biography of Huygens, and the second dealing with a more detailed description of his most important contributions to knowledge.

In the opening sentence of the Preface the author describes Huygens as one of the greatest scientific geniuses of all time and then proceeds to substantiate this statement by enumerating Huygens' accomplishments: he "transformed the telescope from being a toy into a powerful instrument of investigation;" he is "rightly regarded as the founder of the wave theory of light and thus of physical optics;" he founded the dynamics of systems, cleared up the subject of the pendulum and the tautochrone, and discovered the rings and the brightest satellite of Saturn.

In reading this absorbing account of Huygens' life one cannot help but gain the conviction that it was this very versatility that prevented Huygens from reaching the heights that Newton did, though the same versatility formed the basis for the wide acclaim accorded him by his contemporaries. Originally steeped in Cartesian philosophy—Descartes was an occasional visitor in the home of his father—Huygens gradually emancipated himself from that point of view to such an extent that in his later years his position and his accomplishments were much abused by the then remaining proponents of Descartes in Paris.

Huygens' eminence naturally led to his attaining a guiding, and even commanding, position in the Académie Royale des Sciences, founded by Louis XIV (at the instigation of Colbert) in 1666. Reading of the difficulties encountered in its establishment reminds us only too vividly of the very similar birth pangs attending the establishment (we hope) of the National Science Foundation in our own day.

How Huygens was instrumental in having Roemer's discovery of the finite velocity of light accepted over Cassini's opposition; how he was obviously familiar with and used what are now known as Newton's first and second laws of motion; how he observed the rotation of the planet Mars, but, with characteristic caution and distrust of the accuracy of his own observations, delayed publication of it; how, regardless of Da Vinci's and Galileo's earlier work, it was Huygens who made the pendulum clock into a practical reality—all these things we are in need of being reminded of, for in spite of his great accomplishments it is "the accidents of history" which, as the author shows, are responsible for Huygens' not having attained a stature equal to that of Galileo and Newton.

The second half of the book goes into considerable detail on Huygens' scientific work, dealing especially with the pendulum clock and his wave theory of light and concluding with an evaluation of Huygens' place in the history of science. Appropriately, the clue to his own conception of scientific research is presented in his own words: the difficulties in the problems cannot be overcome "except by starting from experiments and then by conceiv-

ing certain hypotheses"—which would still be sound advice to any prospective scientist today.

W. J. LUYTEN

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The Growth of Physical Science. Sir James Jeans. x + 364 pp. $4.00. Cambridge, at the University Press. Macmillan. New York.

PARALLELING the growth of physical science itself, this book moves leisurely through ancient and medieval times, then with increasing tempo arrives in the twentieth century with the same "breakneck speed" with which the author characterizes today's motion of "our material civilization."

This increasing tempo results obviously from the confinement of the total story within the covers of a single book. But the latter pages could perhaps have been put to a more useful purpose had the author granted the difficulty of an adequate coverage of the growth of modern physical science, and instead, with a few examples, sought to present the same perspective in modern times which he achieves so well in his portrayal of the growth of physical science in Greece, Alexandria, and even during the Dark Ages.

The continuous impact of this early history is seen forcing science from one center of learning to another and eventually into hiding. The tolerance of the Romans allows the University of Alexandria to flourish, but later the University declines as Christianity "must have provided a powerful deterrent to the scientific spirit of free inquiry." Some of its scholars migrate to Byzantium before its final end when the Mohammedans conquer Alexandria.

Later, physical science is seen taking root in Western Europe, but we learn nothing of the manner in which it subsequently spans the Atlantic. The slowness of America to achieve a significant role in the growth of physical science, as well as the migration of European scientists to America during the past few decades, receives no such analysis as do the early meanderings of science about the Eastern Mediterranean.

At the outset, Jeans states that he hopes "to trace out the steps by which it [physical science] has attained to its present power and importance," but nowhere does he mention the mutual invasion of the practical arts and physical science which has occurred during the past century. He does refer occasionally to practical contributions to science, such as the invention of printing and of the telescope. He may be right when he notes some of the writings of Francis Bacon and goes on to say that seventeenth-century science "no doubt lost much through this shifting of the emphasis from knowledge for its own sake to knowledge for utility's sake." But surely modern technology has had a sufficiently important role in the attainment of physical science "to its present power and importance" to at least deserve mention.

The book is intended for the educated layman, but even for those readers well acquainted with present-day high-school mathematics, it is not always light reading. The thinking of many of the great minds in the growth of mathematics, physics, chemistry, and astronomy is clearly described. The author obviously delights in presenting the reader with some of the mathematical problems solved by Euclid, Leonardo, and others, but the reader is never left in doubt, for the solutions are always provided.

PHILIP N. POWERS

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Washington, D. C.

The Green World of the Naturalists. Victor Wolfgang von Hagen, Ed. xix + 392 pp. $5.00. Greenberg. New York.

CONSIDERING the extent of the literature on the natural history of South America, Mr. von Hagen has chosen wisely for his splendid anthology. The selections, arranged in chronological order and carefully annotated, cover the continent from Panama to Tierra del Fuego, from Brazil to the Galápagos. The subjects include trees, plants, fruits, animals, insects, birds, reptiles, fish, and even geology and anthropology.

The early reports of the world behind the "Green Curtain" are enchanting for the archaic spelling and quaint expressions, and equally so for the amazingly acute—although not always accurate—observations. Pietro Martire d'Anghiera opens the book with his fifteenth-century Of the Supposed Continent. Next comes Gonzalo Fernández de Oviedo y Valdés with a wondrous description of ant (eaters) bears extracting termites from a nest of "suche hardeness that it may seeme a stronge pavement." Then José de Acosta ponders—close to the later-developed theory of evolution—"how it should be possible that at the Indies there should be any sortes of beasts, whereof the like are no where else."

Exactly as it was impossible to include all the great ones in this book, so it is difficult here to do more than hint at the contents. There is Félix de Azara's remarkable story of the wild horses of the

Argentine and his eighteenth-century interest in genetics. You will find Charles Robert Darwin's account of the inhabitants of Tierra del Fuego in 1832. Alcide Dessalines D'Orbigny writes the beautifully poetic story of his travels. There is Henry Walter Bates for army ants, ants, and termites; Richard Spruce for rubber collecting and fabricating; Edward Whymper with a marvelous ascent of the towering mountains of Ecuador; William Henry Hudson in Patagonia; William Beebe on Falling Leaves; and H. M. Tomlinson in the "infinite, lofty" jungle. You would expect a fine article on birds from Frank M. Chapman; there is, too, his Making of a Cayuco. The latter part of the book is rich in fact, not only in natural science but also in literary merit. Thus, the final selection, Ivan T. Sanderson's story of monkeys, grison, opossum, and jaguar, is superb. The book as a whole is an interesting—even thrilling—addition to any library.

MARJORIE B. SNYDER
Washington, D. C.

The Royal Society: Newton Tercentenary Celebrations. xv + 92 pp. $3.00. Macmillan. New York.

PROBABLY no name in the chronicles of science is more universally recognized than that of Sir Isaac Newton, born at Woolsthorpe in 1642. His many contributions to mathematics, mechanics, and optics are probably overshadowed in the public mind by his outstanding discovery of the universal law of gravitation.

It is eminently fitting that the three-hundredth anniversary of the birth of this genius should have been celebrated in London and Cambridge. Postponed because of the war, the Tercentenary Celebrations actually occurred in July 1946. The volume is published under the auspices of the Royal Society of London, at whose invitation the national academies of science of the world joined in paying homage to Newton's memory.

In less than one hundred pages there are contained the program of the celebrations and the following addresses: Address of Welcome to the Delegates, by the President of the Royal Society; Newton, by Professor E. N. da C. Andrade; Address of Welcome to the Delegates, by the Master of Trinity (Dr. G. M. Trevelyan); Newton, the Man, by the late Lord Keynes (read from Lord Keynes' ms. by Mr. Geoffrey Keynes); Newton and the Infinitesimal Calculus, by Professor J. Hadamard; Newton and the Atomic Theory, by Academician S. I. Vavilov (read on behalf of the author by Sir Henry Dale); Newton's Principles and Modern Atomic Mechanics, by Professor N. Bohr; Newton: the Algebraist and Geometer, by Professor H. W. Turnbull; Newton's Contributions to Observational Astronomy, by Dr. W. Adams; and Newton and Fluid Mechanics, by Professor J. C. Hunsaker.

Throughout, one is reminded of the human traits of an individual who was at once a scientist and a man of affairs. It is a bit surprising to those less well acquainted with Newtonian literature to find that so many of his outstanding attainments represented so small a proportion of his lifetime career.

It is gratifying to note that the Royal Society has taken steps toward the establishment of an Isaac Newton Observatory to contain a 100-inch reflecting telescope that, it is hoped, will be granted by H. M. Government and that will be made accessible to the personnel of university observatories for the purpose of astronomical investigations, to which Sir Isaac Newton so significantly contributed.

The book contains six illustrations and is an important contribution to the history and bibliography of science.

HARLAN T. STETSON
Cosmic Terrestrial Research Laboratory
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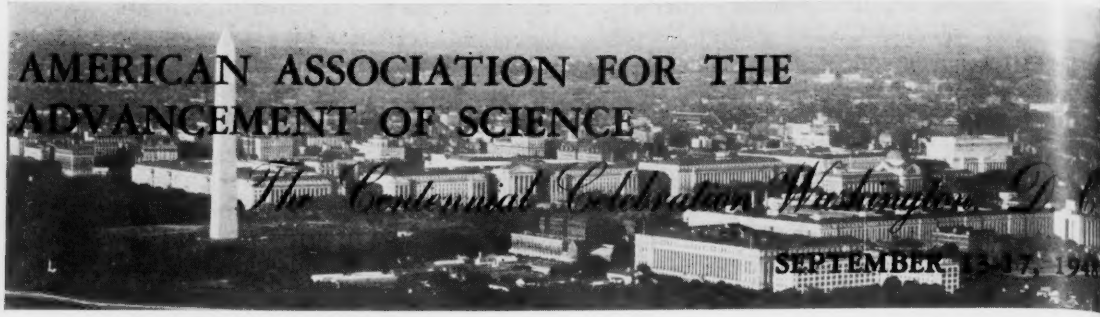

AFTERNOON ACTIVITIES

An unusual feature of the Centennial Celebration in September will be the opportunity to see "One World of Science" as it is illustrated in action in the nation's capital. The government of the United States has a tremendous investment in scientific research, and, although a major part of it is carried on elsewhere, it is only in Washington and its vicinity that examples of nearly every type may be found.

More than forty institutions are cooperating in arranging local functions, and they will be hosts to the visiting scientists. These institutions include American University, George Washington University, Georgetown University, Howard University, and the University of Maryland; societies affiliated with the AAAS who have headquarters in or near Washington: Academy of World Economics, American Association of Economic Entomologists, American Chemical Society, American Home Economics Association, American Pharmaceutical Association, American Psychological Association, American Statistical Association, Metric Association, National Association of Science Writers, National Education Association, Society of American Foresters, and Society of Rheology; governmental organizations: Bureau of Agricultural Economics, Bureau of Animal Industry, Bureau of Entomology and Plant Quarantine, Forest Service, U. S. Coast and Geodetic Survey, Weather Bureau, National Bureau of Standards, Bureau of Mines, Fish and Wildlife Service, Food and Drug Administration, National Institute of Health, U. S. Office of Education, U. S. Public Health Service, David Taylor Model Basin, National Naval Medical Center, Naval Ordnance Laboratory, Naval Research Laboratory, National Academy of Sciences, Office of Naval Research, U. S. Naval Observatory, National War College, Library of Congress, and Smithsonian Institution; also, Carnegie Institution of Washington and the National Society of the Daughters of the American Revolution.

For relaxation, those attending the Celebration will have an opportunity to make unscheduled trips to the many art galleries, libraries, museums, and historic monuments located in Washington. A guide to these and a map of the city of Washington will be provided at the time of registration. Since no activities have been scheduled for Monday afternoon, September 13, those arriving in Washington early may avail themselves of this opportunity to study the superb cultural displays situated along the Mall. The opening session, in Constitution Hall, will not begin until 8:30 P.M.

Special tours of institutions engaging in scientific research are being planned for the afternoons of Tuesday, Wednesday, and Thursday, September 14–16. The National Institute of Health, Agricultural Research Center at Beltsville, National Naval Medical Center, National Bureau of Standards, and many others will welcome visiting scientists and their families, and conduct them on inspection trips through laboratories housing demonstrations that feature current research programs. Those who wish to participate in these tours may make their reservations at the time of registration. A schedule of trips to institutions will be published in the forthcoming announcement of the meeting soon to be mailed to members of the Association.

There will be much to attract specialists who may desire to see advancements in their own fields of science, but the important purpose of the afternoon activities will be to enlarge the horizons of all who participate in the Centennial Celebration, to bring together educators, research workers, and others interested in advancing human welfare.

As chairman of the Afternoon Activities Committee, it gives me great pleasure to invite the members of the Association and the members of its affiliated societies to join in the commemoration of the founding of the AAAS, and to extend to them a cordial invitation to visit the great scientific and cultural institutions in and about Washington.

Raymund L. Zwemer

Executive Secretary, National Academy of Sciences, and Chairman, Afternoon Activities Committee, AAAS